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ADAPTATION IN ISOLATED COLD SPOTS

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Systematic studies of thermal adaptation have been made almost entirely with areas of several square centimeters of skin tissue or larger.[1] Comparatively little is to be found in the literature concerning adaptation in single temperature spots. Yet if thermal sensitivity is assumed to be punctiform in nature, the single temperature spot would seem to be the logical starting point. This is particularly true because adaptation effects have played such a critical rôle in the development of theories of temperature sensitivity.

In areas of any considerable size, adaptation is generally found to be slow and incomplete; but is it safe to assume that this will be true with a single temperature spot? There are hints in the literature that such may not be the case. Many investigators have noted that stimulation of a temperature spot seems to depress its sensitivity rather rapidly. In mapping experiments, it is the traditional caution to avoid stimulating the same spot repeatedly.[2] A number of additional observations are to be found in the

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[1] An excellent bibliography of the studies of thermal adaptation prior to 1921 will be found in Elof Gertz, Psychophysische Untersuchungen über die Adaptation im Gebiet der Temperatursinne und über ihren Einfluss auf die Reiz- und Unterschiedsschwellen, *Zsch. f. Sinnesphysiol.*, 52, 1921, 1-51, 105-156. Important studies since then are: Erich Hummel, Die Veränderung des Systemes der Temperaturempfindungen durch die Adaptation, *Arch. f. d. ges. Psychol.*, 57, 1926, 305-394; Helmut Hahn, Die psycho-physischen Konstanten und Variablen des Temperatursinnes, *Zsch. f. Sinnesphysiol.*, 60, 1930, 162-232; Solomon Aronoff and K. M. Dallenbach, Adaptation of warm spots under continuous and intermittent stimulation, this JOURNAL, 48, 1936, 485-490; H. A. Levine and K. M. Dallenbach, Adaptation of cold spots under continuous and intermittent stimulation, *idem*, 490-497.

[2] Cf. M. B. Drury and K. M. Dallenbach, The response of cold spots under successive stimulation, this JOURNAL, 43, 1931, 490-496; and also R. H. Earhart and K. M. Dallenbach, The response of warm spots under successive stimulation, *ibid.*, 45, 1933, 722-729.

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works of Goldscheider. He notes that complete disappearance of sensation may result from continuous or intermittent stimulation.[3] The extent and duration of the ensuing depression depend upon the intensity and dura-

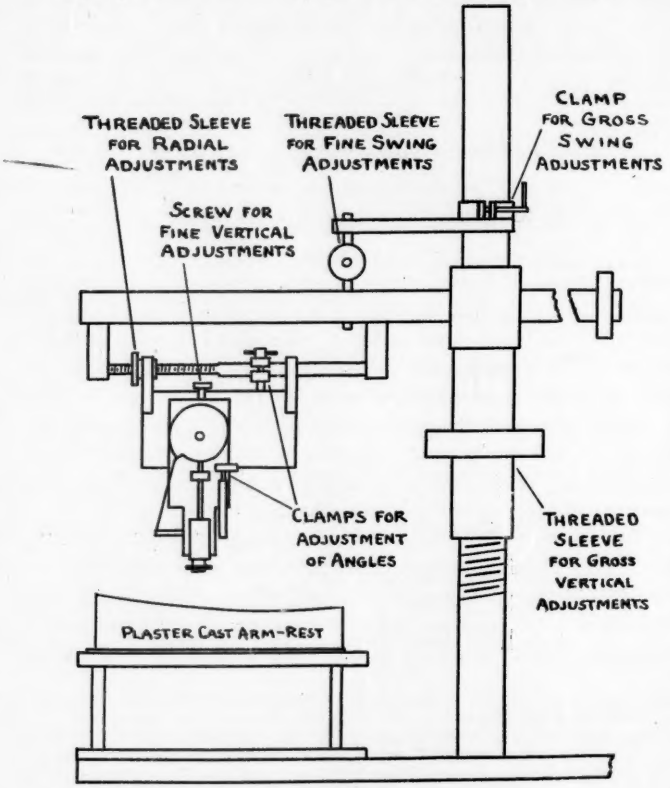
THREADED SLEEVE FOR RADIAL ADJUSTMENTS
THREADED SLEEVE FOR FINE SWING ADJUSTMENTS
CLAMP FOR GROSS SWING ADJUSTMENTS
SCREW FOR FINE VERTICAL ADJUSTMENTS
CLAMPS FOR ADJUSTMENT OF ANGLES
THREADED SLEEVE FOR GROSS VERTICAL ADJUSTMENTS
PLASTER CAST ARM-REST

FIG. 1. MAIN APPARATUS

tion of the stimulus.[4] Recovery is relatively slow, but the originally most sensitive spots show the most rapid recovery.[5] Warming the skin does not hasten recovery.[6] Unfortunately, all of these are recorded merely as qualita-

[3] Alfred Goldscheider, *Gesammelte Abhandlungen*, 1898, 142 f.
[4] Goldscheider, Temperatursinn des Menschen, in Bethe's *Handbuch der normalen und pathologischen Physiologie*, 11, 1926, 134 ff.
[5] Goldscheider, *Gesammelte Abhandlungen*, 1898, 152.
[6] *Ibid.*, 143.

tive statements, without specific figures which could be used as a basis for comparison with the usual results for larger areas.

In the following study, adaptation effects in single cold spots have been studied systematically. Furthermore, a technique has been used whereby

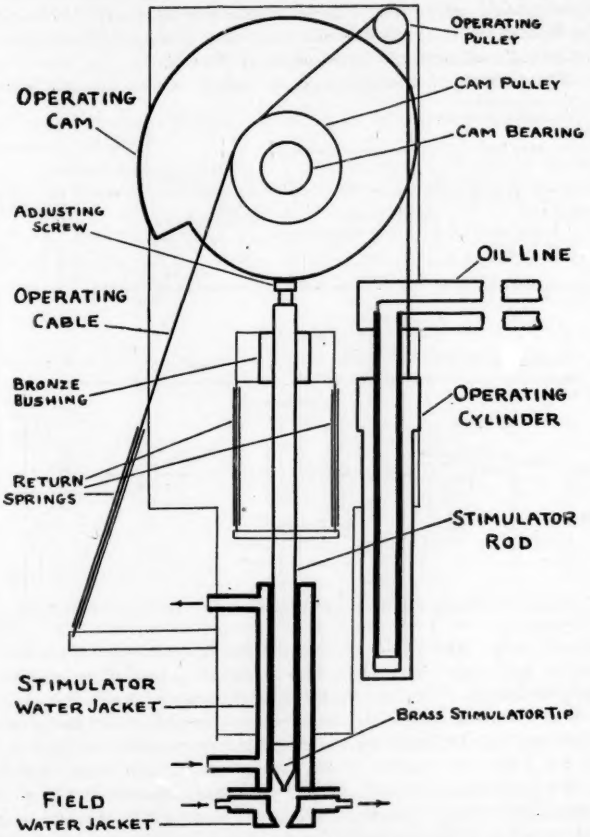

FIG. 2. DETAIL OF THE MECHANISM OF THE STIMULATOR-FIELD

simultaneous tactual stimulation is excluded, since it is believed that touch may complicate and confuse accurate thermal discrimination.

METHOD AND PROCEDURE

Apparatus. The apparatus used in this investigation is structurally complex, but functionally rather simple. It provides a means of surrounding a cold spot with a

field 16 mm. in diam. and stimulating it with a metal tip 1.25 mm. in diam. The temperatures of the field and of the stimulator-tip may be varied and measured independently. The stimulator-rod is moved down by a cam so that the tip comes just flush with the surface of the field disk. Hence, there is normally no distortion of the skin and no tactual stimulation to distract *O*.

Fig. 1 shows the main apparatus and the method of supporting and controlling the stimulator-field mechanism. The mechanism may be clamped at any desired angle so that the surface of the field disk lies tangent to the skin. It may be adjusted both horizontally and vertically to the necessary position.

Fig. 2 is a simplified drawing of the mechanism of the stimulator-field. When

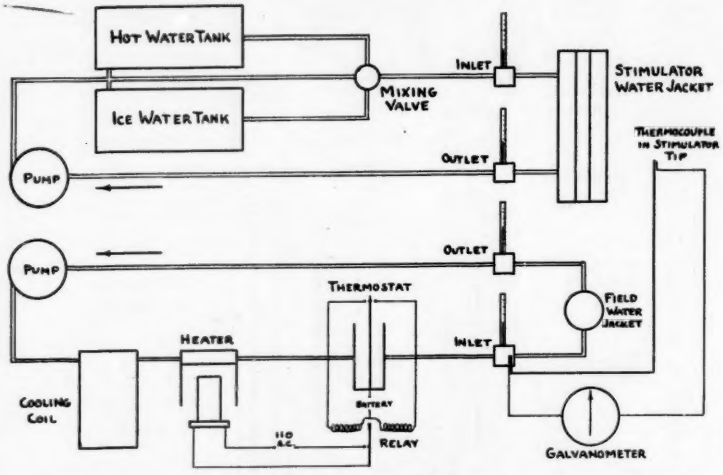

FIG. 3. SCHEMATIC DIAGRAM OF TEMPERATURE CONTROL AND MEASUREMENT

pressure is applied through the oil line, the operating cylinder moves downward, turning the cam via the operating cable. At the other end of the oil line is the operating mechanism. When the experimenter depresses a handle, this mechanism: (a) sounds a warning buzzer; (b) introduces a delay of 2 sec. by means of a dash-pot timer; (c) then applies pressure to the oil line by a system of weights.

Fig. 3 is a schematic diagram of the system of temperature control and measurement. The field-temperature may be: (a) maintained thermostatically at 33° C.; (b) gradually raised by manual control of the heater; (c) gradually lowered by manual control of the cooling coil. The field-temperature is measured by inlet and outlet thermometers; in practice only the inlet temperature being recorded regularly.

The stimulator-temperature is controlled by varying the temperature of the water pumped through the stimulator jacket. The mixing valve permits either hot water or ice water to be mixed with the water flowing through the system. The temperature of the stimulator-tip is measured by a thermocouple-galvanometer system. One junction of a constantan-copper thermocouple is in the stimulator-tip; the other

junction is in the jacket for the field inlet thermometer. The galvanometer scale is calibrated to read degrees C. directly. Inlet and outlet thermometers provide a check on the galvanometer readings. Reaction-times are measured by the typical break-and-break operation of a synchronous motor chronoscope.

Mapping and marking. At the beginning of an experimental series, several sensitive cold spots were located and marked with a drop of ink. A ring 16 mm. in diam. was then drawn around each spot, using a bow-pen with a mixture of silver nitrate solution and indelible ink. Since the same spots were worked on repeatedly for several weeks or months, these rings required renewal from time to time.

Experimental routine. The typical procedure in securing data on a cold spot may be outlined as follows.

(1) O's left arm was supported in a half plaster cast (made individually for each *O*), with the volar surface approximately horizontal. The mechanism was then adjusted so that the field disk coincided with a marked ring and pressed lightly and evenly on the skin. The spot was then stimulated once with a temperature of about 12° C. Readjustment of the position of the mechanism was made in case this exploratory stimulus did not give the normal intensity of sensation.

TABLE I

TYPICAL DETAIL RECORD ILLUSTRATING THE PROCESS OF DETERMINING THE NORMAL LIMINAL TEMPERATURE

Stimulus-temperature	Reaction-time (millisec.)	Report
28.0°	—	0
24.3°	468	1
25.9°	451	1—
28.0°	—	0
26.6°	476	1—
27.7°	—	0
27.2°	—	0

(2) After a rest of 5 min., the normal liminal temperature[1] was determined by stimulating with a series of temperatures just above and below this level, the field temperature being maintained thermostatically at 33° C. *O* reacted when he felt any cold sensation and reported its intensity in terms of the following code: 3 for strong cold; 2 for moderate cold; 1 for weak cold; 0 for no sensation. Several *O*s introduced intermediate degrees of their own accord. The record in Table I is a typical example of the procedure of determining the normal liminal temperature, in this case approximately 26.5° C.

(3) The conditions to be tested were then introduced. In general, these were continuous or intermittent stimulation at 10°, 20° and 40° to 46° C.; or changes in the field temperature. Sometimes two or more conditions could be tested in the same run. The effects were studied principally by following the course of the liminal temperature, stimulating at 30-sec. intervals. The accuracy of such a method, of course, depends upon how well the experimenter is able to select testing temperatures

[1] To avoid involved phraseology, the term 'liminal temperature' is used to designate the highest temperature of the stimulator which was effective in eliciting a cold sensation.

which lie alternately on each side of the liminal temperature. Fortunately, in all these experiments the effects were usually so clear-cut that the general course could be plainly indicated by this method.

Limitations of the method. In general, it is believed that the procedure outlined was adequate for the problem. It is desired, however, that the limitations of the method be clearly appreciated. The most important of these are:

(1) Errors in re-location. The marked ring seldom remained a true circle from day to day, but was usually distorted into various elliptical shapes.[8] Hence, there is no assurance that a given spot was stimulated in exactly the same way from period to period. In practice, a purely empirical criterion was used: the exploratory stimulus must give rise to the normal clear cold sensation characteristic of the particular spot.

(2) Limits of temperature measurement. Allowing for the temporal lag of the measuring devices and the errors of calibration and reading, it is believed that the temperatures of the stimulator and field as given are correct within plus or minus one degree. Any errors of this size would not affect the general implications of the results; but might be important in the consideration of absolute values; for example, the maximum temperatures to give a cold sensation with normal and elevated field temperatures.

(3) Limits of time control. The duration of stimulation was determined by the length of time *E* held down the operating lever, and was necessarily subject to some variation. This is chiefly important in short durations, as in testing for the liminal temperature and in intermittent stimulation, where the stimulator-tip was in contact with the skin for one second or less. With stimulus-durations of several seconds or more, the variations would be of little consequence.

RESULTS

With the apparatus and procedure previously described, complete or partial data were obtained on some 30 cold spots on the volar surface, left forearm, of 6 *Os*.[9] All were young men students, psychologically naïve as far as a knowledge of the facts of temperature sensitivity was concerned. They were kept in ignorance of the purpose of the problem and the trend of the results. The experiments were performed between March 20 and May 21, 1935, at different times of day and under a variety of preceding environmental conditions. The room temperature was usually 22-23° and the typical test period one hour in length.

(A) With field at 33° C. (1) Normal limen. The solid squares in Fig. 4 indicate the normal liminal temperatures on various occasions for 20 different cold spots on 6 *Os*. Each vertical column contains the data for one

[8] For similar volumetric changes see K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 407 f.

[9] Our thanks are due Miss Margaret Ives and Mr. Charles Irwin for acting as *O* and *E* respectively in the preliminary experiments, and to Messrs. Bolitho, Clark, Malewitz, Richter, Riker, and Sajkowski for acting as *Os* in the main experiments. Our thanks are also due Professors Carl R. Brown and B. D. Thuma for their aid in constructing the apparatus and in developing the method.

cold spot, the letters and digits at the top indicating the *O* and identification number of the spot. There are definite indications of individual differences among cold spots and among the *O*s, which may account for the discrepancies in the results reported in the earlier literature. Goldscheider,[10] for instance, found some cold spots on his own left forearm which responded to 32°, and more responding to 30.5°. Siebrand,[11] on the con-

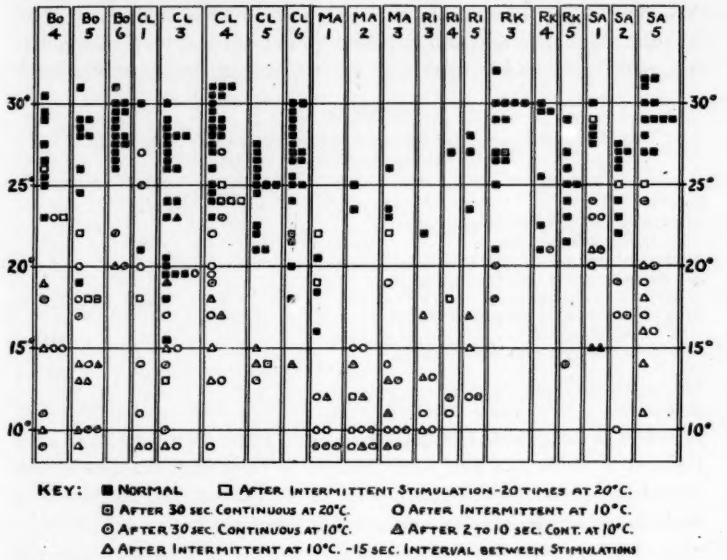

FIG. 4. NORMAL AND DEPRESSED LIMINAL TEMPERATURES
(Each column gives the data for one cold spot.)

trary, found in a similar area on his *O* no spots with a normal liminal temperature above 29.5° and the majority in the range of 27° to 28°. Apparently the sensitivity of some single cold spots is nearly as great as that of larger areas; although Goldscheider has shown that an area of several square centimeters will respond to a temperature which will not excite any of the individual spots within it.[12]

[10] Goldscheider, Beiträge zur Lehre von der Hautsensibilität, *Zsch. f. klin. Med.*, 74, 1921, 274.
[11] —. Siebrand, Untersuchungen über den Kältesinn, *Zsch. f. Sinnesphysiol.*, 45, 1911, 208.
[12] Goldscheider, *op. cit.*, *Zsch. f. klin. Med.*, 74, 1912, 280.

Individual differences in variability among the different cold spots can also be noted. The liminal temperatures determined in the same spot on various occasions are closely grouped in some spots (as *Bo*-6 and *Sa*-5) and are quite scattered in others (as *Cl*-3 and *Cl*-6).

(2) Depression by low temperatures. In general, the after-effect of stimulation by low temperatures, either intermittent or continuous, was a long-lasting depression of the liminal temperature. The symbols in Fig. 4 show these depressed levels in accordance with the key at the bottom of the chart. As these results indicate, the effects of different types of stimulation varied considerably from spot to spot, and in the same spot from

TABLE II

AVERAGES OF NORMAL AND DEPRESSED LIMINAL TEMPERATURES, OBTAINED BY COMBINING RESULTS FOR ALL O_3

Condition	Mean	A. D.	Range	No. cases
Normal	26.4°	2.7°	15½–32°	167
After 20 intermittent brief stimulations at 20°, 30-sec. intervals	21.7°	3.2°	12–29°	18
After 30-sec. continuous stimulation at 20°	20.3°	4.3°	14–27°	4
After 20 intermittent brief stimulations at 10°, 30-sec. intervals	15.3°	3.9°	9–27°	47
After 20 intermittent brief stimulations at 10°, 15-sec. intervals	14.1°	2.6°	9–21°	24
After 2–10 sec. continuous stimulation at 10°	14.5°	3.0°	9–23°	18
After 30-sec. continuous stimulation at 10°	15.3°	5.0°	9–25°	34

period to period. These facts must be kept in mind when considering the averages in Table II which were obtained by combining the data for all 20 spots in order to give a rough perspective of the general magnitude of the depression. The effect of the stimulus varies with:

(a) The spot tested. For example, witness the drastic effects on *Ma*-2, compared with the mild depression in *Sa*-1.

(b) The stimulating temperature. The depression after 10° is definitely greater than that after 20°.

(c) The interval between stimulations. Apparently intermittent stimulation at 15-sec. intervals produced somewhat greater depression than intermittent stimulation at 30-sec. intervals. This is best shown by considering individual spots in which a number of instances of both types are available (as *Bo*-5 and *Sa*-5).

(d) The duration of stimulation. Apparently this is effective only within certain limits, since durations of 30 sec. produced only slightly greater depressions than durations of 2-10 sec., and these only on weaker spots.

The symbols in Fig. 4 indicate in each case the lowest level of the depression following stimulation by low temperatures. The course of the ensuing recovery was determined only under the conditions of testing every 30 sec. to determine the liminal temperature. Under these circum-

stances, recovery was found to be relatively slow—not faster than 1° per min., and usually much slower. This is partially an artifact. Every stimulation would tend to produce its own depressing effect, thus artificially delaying recovery. Hence, when the depression was extreme and testing temperatures correspondingly low, frequently no recovery at all was evidenced. With complete rest, the rate of recovery would undoubtedly be higher, but systematic determinations of this kind were not undertaken, because of the great amount of time such a process would consume.

Goldscheider holds that the depression following stimulation is preceded by a brief period of hyper-sensitivity.[18] Possibly because of a difference in

TABLE III

TYPICAL RECORD FROM A 'FADING' SPOT

Order	Stim.-temp.	Reaction-time (Millisec.)	Report	Order	Stim.-temp.	Reaction-time (Millisec.)	Report
(1)	20.4	490	1	(10)	17.9	—	0
(2)	23.9	—	0	(11)	17.7	—	0
(3)	20.6	—	0	(12)	17.1	583	1
(4)	19.6	—	0	(13)	16.8	—	0
(5)	17.7	461	1	(14)	16.4	—	0
(6)	18.3	520	1	(15)	14.4	—	0
(7)	18.0	538	1	(16)	12.3	—	0
(8)	18.3	—	0	(17)	10.5	—	0
(9)	18.0	537	1				

technique, the present study does not confirm this finding. Tests made 30 sec., even 15 sec., after cessation of stimulation showed only marked depression.

(3) 'Fading.' The depressive effects of approximately liminal (and possibly even sub-liminal) stimulation are illustrated by the phenomenon of 'fading.' This term is used to describe a swift downward trend of the liminal temperature under the mild stimulation incidental to determining the normal level. One such instance is shown in Table III. Sometimes the plunge was even more rapid than this. Frequently, within half a dozen stimulations at 30-sec. intervals, such a spot would fail to respond to 10°, although the response to the original exploratory stimulus had been of quite normal intensity. 'Fading' was a frequent occurrence in some spots and was never observed in others. None of *Sa*'s spots ever showed it; whereas all of *Ri*'s faded consistently every time tested during the last three weeks of the experimental period.

In determining the liminal temperature, we are apparently dealing with a 'principle of uncertainty.' To find the liminal temperature, it is necessary

[18] Goldscheider, Temperatursinn des Menschen, in Bethe's *Handbuch der normalen und pathologischen Physiologie*, 11, 1926, 134.

to stimulate the spot, but this stimulation in itself may cause a depression; hence uncertainty. Fortunately, in most spots, if the testing temperatures are kept close to the liminal level, this depressing effect seems to be slight.

TABLE IV

RESTORATIVE EFFECT OF HIGH TEMPERATURES

Columns A = Liminal temperature just before contact with high temperature.
Columns B = Liminal temperature after 1 min. contact with stimulator-tip at 40°–46°.
Columns C = Liminal temperature five minutes later.

O and spot	A	B	C	O and spot	A	B	C	O and spot	A	B	C
Bo-5	31°	32°	32°	*Cl-6*	27°	30°	29½°	*Ri-6*	10°	21½°	10°
Bo-5	32	32		*Cl-6*	20	29½	24	*Ri-6*	10	20	11
Bo-5	10	22	13	*Cl-6*	14	30½	30½	*Ri-6*	10	13	10
Bo-5	18	29	27	*Cl-6*	17	30½	29	*Ri-6*	10	20	11
Bo-5	18	26	22	*Cl-6*	15	25	16	*Ri-6*	10	21	11½
Bo-5	15	25	13	*Cl-6*	12	21½	11	*Ri-6*	10	19	11
Bo-5	13	14	11	*Cl-6*	11	26½	11				
Bo-5	19½	27	9	*Cl-6*	10	21½	10½	*Rk-1*	16	19	9½
Bo-5	10	22	11	*Cl-6*	9	20½	10				
				Cl-6	10½	20½	11	*Rk-2*	19	25½	23
Bo-6	21	29	26	*Cl-6*	11	26	11	*Rk-2*	23	28	23
Bo-6	23	29	26	*Cl-6*	10½	19½	10½				
Bo-6	13	25	20	*Cl-6*	11	19	11	*Rk-3*	18	31½	
				Cl-6	11	20½	11	*Rk-3*	23½	31½	
Cl-1	10	21	10	*Cl-6*	11	20½	11½	*Rk-3*	11	24	24
								Rk-3	10	19	10½
Cl-3	28½	30½	31	*Ma-1*	18½	19½	17				
Cl-3	27	31½						*Rk-4*	21	28	26½
Cl-3	31½	31½		*Ma-2*	14	11	11	*Rk-4*	10½	10½	
Cl-3	19½	25	25	*Ma-2*	14	25	22				
Cl-3	18	27	24	*Ma-2*	22	29½		*Rk-5*	14½	25½	20½
Cl-3	13	27½	24								
Cl-3	10	13	11	*Ma-3*	26	29	26	*Sa-1*	23	31	
				Ma-3	10	16	11½	*Sa-1*	24	31	
Cl-4	27	31	30	*Ma-3*	13	20½	16½				
Cl-4	30	31	31					*Sa-2*	17	27½	25
Cl-4	28	30	30	*Ri-3*	9	17	11	*Sa-2*	19	24½	
Cl-4	11	24½	11								
				Ri-4	12	27	10	*Sa-5*	24½	30½	30½
Cl-5	26	31	28					*Sa-5*	30½	31½	
Cl-5	25	27	27	*Ri-5*	17	28	26	*Sa-5*	20	30	30
Cl-5	27	28	28	*Ri-5*	12	28	19½				
Cl-5	15	22	17	*Ri-5*	10	15	10	*Sa-7*	19	28	26
Cl-5	13	23	16	*Ri-5*	10	16	10½				
Cl-5	10	20	19	*Ri-5*	10	18	10½				
Cl-5	12	23	11	*Ri-5*	10	19	10				
Cl-5	10	15		*Ri-5*	11	19	13				
Cl-5	10	20	11								

In many instances, it was possible to stimulate a dozen times or more with temperatures close to the limen, without affecting its level.

(4) Restoration by high temperatures. More striking than the depression by low temperatures, is the rapid restoration of the liminal temperature by one minute's contact with a stimulator tip at 40° to 46°. Table IV

shows the effects secured on various occasions in twenty-three different spots. Column A is the liminal temperature just before the high temperature was applied. Column B shows the corresponding liminal temperature after one minute's contact with a stimulator tip at 40° to 46°. Column C shows the liminal temperature 5 min. later.

Notice that even an extremely depressed limen can be restored temporarily to somewhere near its normal level. Where the depression has not been too great, this restored level may be reasonably well maintained. After extreme depression, however, even mild stimulation typically causes a slump, and within 5 min. the limen is back at its depression level. This secondary

TABLE V

DURATION OF THE SENSATION WITH CONTINUOUS STIMULATION OF A SINGLE COLD SPOT AT A CONSTANT TEMPERATURE

Duration	No. cases	Percentage
Less than 2 sec.	20	21
From 2–4 sec.	53	57
From 4–6 sec.	10	11
Over 6 sec.	10	11

slump is particularly precipitate in spots restored after 'fading.' Yet the treatment proved singularly effective in almost every case, and became the standard method of resuscitating spots that had 'faded.'

(5) Restoration by rest periods. Following extreme depression, rest periods of 2–5 min. were sometimes introduced. After such a rest, the spot would usually respond—once, twice, rarely more—even to considerably higher temperatures. Then it was 'out' completely to the lowest testing temperatures available. This restoration was evidently more temporary than that brought about by high temperatures. The two seem, however, to have one point in common; in both cases, it is the liminal temperature of the spot that is restored and not its stability. Rest periods longer than 5 min. were not tested.

(6) Duration of cold sensations. Tests were made with continuous stimulation at 10°, 15°, 20°, and 25°, to determine the time needed to reach complete thermal indifference with constant temperature. In these tests, the *O*s were instructed to release the reaction-key when the sensation died out, thus measuring the elapsed time from the instant of contact of the stimulator-tip. In these discriminations, the absence of simultaneous tactual stimulation was a distinct aid; for, when the cold sensation died out, there was nothing left. The results showed a surprising agreement. At all temperatures and for all *O*s combined, the durations of the cold sensations were distributed as shown in Table V. (The disappearance of the cold sensation was not caused by a warming-up of the stimulator-tip, since the

temperature of this never rose more than a degree or two during the entire period of contact.) In three-quarters of the cases, complete adaptation occurred in less than 4 sec. Only in the single instance of *Sa* were durations longer than 10 sec. ever experienced, and many of his were shorter than 4 sec. All of the times are so short that it is impossible to say whether there is a definite relation between the adapting temperature and the time needed to reach complete thermal indifference, such as has been found with larger areas.

The chief point of interest here is that durations averaging less than 4 sec. are of a totally different order of magnitude from those reported by Holm[14] and Gertz[15] with an area of about 5 sq. cm. Holm used a metal stimulator 5 cm. in diam., fastened to the abdomen and surrounded by a ring maintained at skin-neutral. Gertz repeated this experiment on the forearm, and also employed the scheme of anesthetizing a ring of skin around the stimulator to eliminate any effects from the spread of the stimulus. To reach complete thermal indifference, Holm found times varying from 210 sec. at 5°, to 72 sec. at 20°, to 31 sec. at 30°. The values given by Gertz are somewhat longer. With areas as large as the hand, various experimenters have found that complete thermal indifference is not reached in half an hour or more, except with temperatures close to that of the skin.

(7) Reaction-time. Since the measurement of reaction-time was a routine accompaniment of each stimulation, the entire series resulted in the accumulation of well over ten thousand measurements. A detailed presentation of these will be omitted and the essential findings summarized in the following statements.

(a) Every *O* tended to show a basic level of reaction-time, varying from about 300 millisec. for the fastest *O* to about 600 millisec. for the slowest. As might be expected, these reaction-times are much longer than those reported for stimulation of larger areas, which are from 150 to 200 millisec.[16]

(b) Marked deviations of single reaction-times from the general level appear to be referable mainly to central factors. For example, with extremely weak sensations, very long reaction-times sometimes occurred. Frequently, *O* would then report that he had been slow to react because he was not sure at first that he had felt anything.

(c) There is absolutely no evidence, in these cold spots, of any clear-cut relation between the length of reaction-time and stimulus temperature. Table VI shows the averages of reaction-times at 10° and 20° for several spots. Every figure in the table is the mean of 20 to 100 individual measurements, taken in sets of twenty under comparable conditions. The differences between reaction-times at 10° and at

[14] K. G. Holm, Die Dauer der Temperaturempfindungen bei constanter Reiztemperatur, *Skan. Arch. f. Physiol.*, 14, 1903, 245.

[15] Gertz, *op. cit.*, *Zsch. f. Sinnesphysiol.*, 52, 1921, 22-25.

[16] Goldscheider, *Gesammelte Abhandlungen*, 1898, 312-355; Friedrich Kiesow and Mario Ponzo, Beobachtungen über die Reaktionszeiten der Temperaturempfindungen, *Arch. f. d. ges. Phych.*, 16, 1910, 376-396; M. von Vintschgau and E. Steinach, Zeitmessende Versuche über den Temperatur- und Drehsinn, *Arch. f. d. ges. Physiol.*, 43, 1888, 152-194.

20° are negligible, except in the case of *Sa*-5 where the value for 20° is the mean of only twenty readings and the discrepancy may be the result of chance variation.

There is certainly no indication in our results that reaction-times are consistently shorter at the lower temperature. This point is emphasized, because the length of reaction-time has been used by Bazett and McGlone to compute the depth of the thermal receptors, on the basis of the rate of temperature conduction through the skin.[17] If any considerable portion of the reaction-time were required for conduction through the skin, on purely physical principles we should expect to find shorter reaction-times with steeper temperature gradients (lower temperatures). Such is clearly not the case; hence the validity of such computations must be quesitoned.[18]

TABLE VI

AVERAGE REACTION-TIMES AT 10° AND 20°

O and Spot	Av. reaction-time in millisec.	
	at 10°	at 20°
Bo-4	439	409
Bo-5	485	501
Cl-4	390	395
Ri-4	302	304
Sa-5	477	398

(8) Sensations at high temperatures. As a by-product of the process of restoring depressed spots by contact with high temperatures, it was expected to learn something concerning the nature of paradoxical cold; but surprisingly, 'cold' was never reported by any of the 6 *Os* as the result of stimulation of cold spots by high temperatures. Out of 153 cases in which the stimulator-tip had a temperature of 41° to 46°, the reports were: 'Nothing,' 90 times; 'Hot,' 33 times; 'Warm,' 24 times; and 'Non-thermal,' 6 times. All equivocal instances, such as those in which the *Os* might have confused simple touch with warmth, are omitted.

It is significant that 'hot' and 'warm' were reported from stimulation of 13 different spots on 5 of the 6 *Os*. The remaining *O* reported either non-thermal sensations or nothing. When an *O* reported 'hot' or 'warm,' and the same stimu-

[17] H. C. Bazett, B. McGlone, R. G. Williams and H. M. Lufkin, Sensation: I. Depth, distribution and probable identification in the prepuce of sensory end-organs concerned in sensations of temperature and touch; thermometric conductivity, *Arch. Neurol. & Psychiat.*, 27, 1932, 489-517.

[18] Likewise questionable is the assigning of differences in cold and warm reaction-times to a difference in the depth of the cold and warm receptors. In some preliminary studies in which the writer acted as *O*, cold reaction-times averaging well under 300 millisec. were secured. In the same area, was found one sensitive warm spot which was tested on several occasions. Reaction-times to temperatures between 38° and 46° averaged well over 900 millisec., yet there was apparently no relation between reaction-time and stimulating temperature. Repeated attempts to obtain reaction-times to warm spots from our six regular *Os* proved unsuccessful.

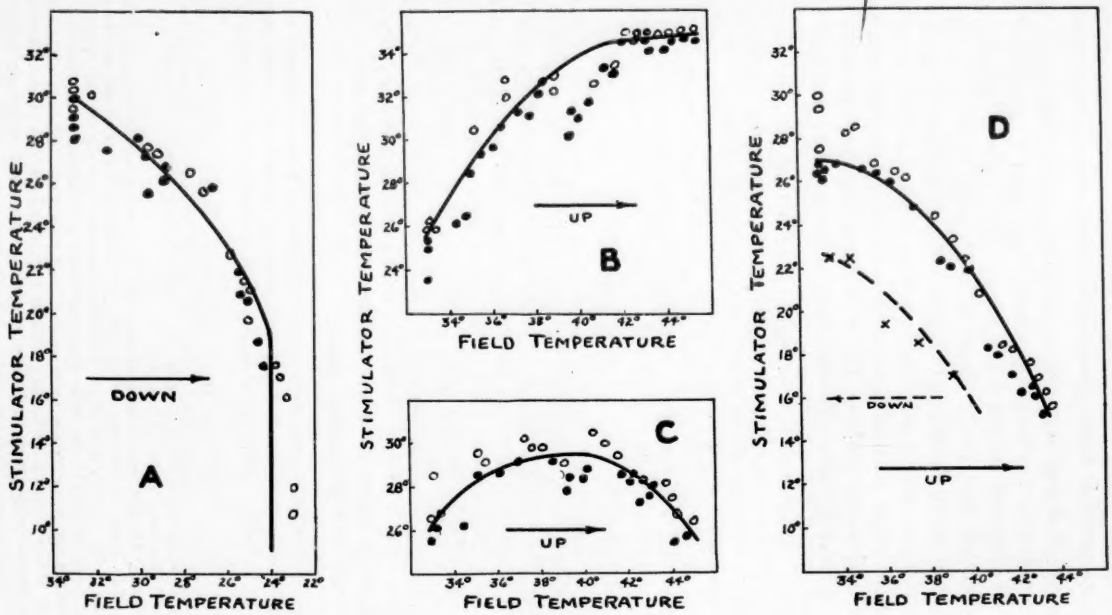

FIG. 5. TYPICAL DETAIL RECORDS WITH RAISED AND LOWERED FIELD-TEMPERATURES

lation was repeated at 30-sec. intervals, he typically felt nothing at all after the first time. Very rarely, a first report of 'hot' might be followed by a report of 'warm' on the second stimulation, and then 'nothing' on the ensuing stimulations.

It is not intended that such reports from psychologically naïve subjects should be taken as adequately descriptive. Yet every *O*, at some time, experienced some sensation at these higher temperatures; and that sensation was certainly not cold. Granting that the 'hot' and 'warm' reports may have resulted from stimulation of adjacent warm spots, the utter absence of paradoxical cold is still arresting.

The one difference in technique that is evident is the presence of the neutral field around the stimulator-tip. This would tend to minimize irradiation. The absence of simultaneous tactual stimulation can scarcely be a differential, because Alrutz succeeded in eliciting paradoxical cold by focusing the sun's rays on a cold spot with a lens.[19]

The reports of 'hot' and 'warm,' recall an almost forgotten study by Kiesow. Employing the usual metal stimulator, he aroused paradoxical cold repeatedly with temperatures between 38° and 40°. He says, "ich habe aber anderseits bei meinen Versuchspersonen kaum einen Kältepunkt gefunden, auf dem nich von einem gewissen Punkte an eine Warmeempfindung beobachtet wurde. Diesser Punkt wurde bei 47° bis 50° C. bestimmt."[20] His description of procedure offers no explanation of why his results should have differed from those commonly obtained. In a later article on apparatus, he speaks of stimulating through holes in a sheet of gutta percha placed over the skin, but does not state whether this device was used in the earlier study.[21] Such a protecting sheet might act similarly to the neutral field around the stimulator-tip in the present study.

Likewise interesting are the results obtained by Heiser, who used an elaborate apparatus for mapping warm spots on the foreheads of 33 *S*s. Although he employed temperatures as high as 47°, reports of 'cold' occurred so rarely that he does not include them in his tabulated results. In Heiser's apparatus, there was no neutral disk nor protecting sheet around the stimulator-tip which was 1.1 mm. in diam.; yet certainly paradoxical cold was a very rare phenomenon. In his summary, he says: "Though no cold stimuli were applied in this experiment, it is certain that some of the spots giving warmth sensation would also have given cold sensation, for in some areas as much as 70-90% of all the spots gave warmth sensation."[22]

(B) With varied field-temperatures. All of the foregoing results were obtained with the field-temperature maintained thermostatically at 33°—or approximately skin-neutral. In the following experiments, the temperature of the field was gradually lowered or raised, and the course of the liminal temperature traced by testing at 30-sec. intervals.

(1) Field-temperature gradually lowered. The detail record shown in Fig. 5 A is typical of the results obtained when the field-temperature was

[19] S. Altrutz, Studien auf dem Gebiete der Temperatursinne, *Skan. Arch. f. Physiol.*, 7, 1897, 333.

[20] Friedrich Kiesow, Untersuchungen über Temperaturempfindungen, *Philos. Stud.*, 11, 1895, 145.

[21] Kiesow, Ein einfacher Apparat zur Bestimmung der Empfindlichkeit von Temperaturpunkten, *Philos. Stud.*, 14, 1898, 589-590.

[22] Florian Heiser, Stimulus-temperature and thermal sensation, *Arch. Psychol.*, 21, 1932, (no. 138), 81.

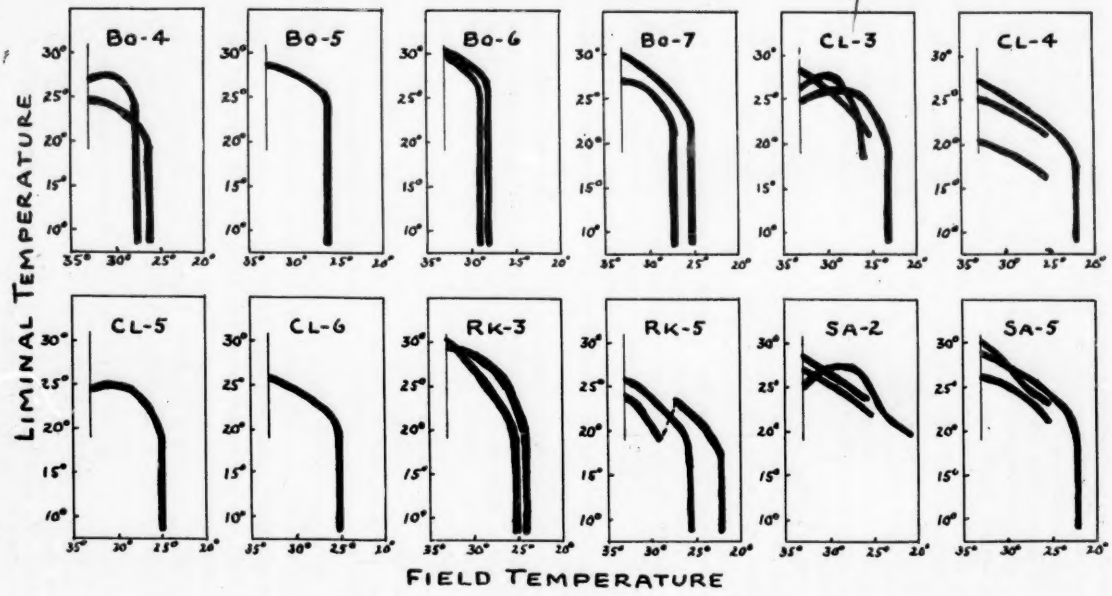

FIG. 6. EFFECTS OF LOWERED FIELD-TEMPERATURE ON DIFFERENT COLD SPOTS

slowly reduced. As the field-temperature drops, the liminal temperature holds its own, relatively, for several degrees. Then quite suddenly, it plunges sharply downward until the spot will no longer respond to the lowest testing temperatures available. Fig. 6 shows the results for a number of different cold spots, each curve being plotted in a manner similar to that shown in Fig. 5 A.

There is an indication that the break occurs at a certain critical level of field-temperature, which is never far below the normal liminal temperature and can scarcely be called excessively low in any case. It is believed that the apparent 'critical level' is genuine. For example, it was found that a stable liminal temperature could be determined with the field at about 28°—only to have the spot fail completely in a few stimulations when the field reached 25°.

What happens at this critical field-temperature in all spots appears to be very similar to what occurs at normal field-temperature in 'fading' spots. For the first test or two, the liminal temperature may be quite high, but it does not stand up under repeated stimulation. Succeeding tests indicate a lower and lower liminal level, until the spot is 'out' even for 10°. Just as with 'fading' spots, the liminal level may be restored temporarily by contact with a stimulator-tip at about 45°, but a secondary slump quickly follows even with mild stimulation.

(2) Field-temperature gradually raised. When the temperature of the field is slowly increased, no such simple and consistent result is obtained. Instead a variety of effects may occur. In Fig. 5 B, C, and D are shown detail records of three typical reactions.

In the first type of reaction (Fig. 5 B), the liminal temperature of the spot rises to a certain maximum value, as the field-temperature is increased. This was 37° to 38° for *Sa;* somewhat lower for the other *O*s. This rise was the expected result, on the basis of the findings of Goldscheider[23] and Siebrand[24] who showed that the liminal temperature for cold could be raised by pre-warming the skin. The maxima obtained in the present study are somewhat higher than those given by these two experimenters.

Less expected was the second type of reaction (Fig. 5 C). That is, as the field-temperature rises, the liminal temperature of the spot rises initially, only later to reverse its trend and eventually come back approximately to where it started. This gives a definite indication that the effect on the limen is not a simple matter, but possibly the resultant of two or more influences.

[23] Goldscheider, Beiträge zur Lehre von der Hautsensibilität, *Zsch. f. klin. Med.*, 74, 1912, 283.
[24] Siebrand, *op. cit.*, 208.

Totally unexpected was the third type of reaction (Fig. 5 D). In this case, there is no rise of the liminal temperature at all. Instead, as the field-temperature is increased, the liminal temperature goes steadily downward until finally the spot may fail to respond even to 10°. Now if the field-temperature is gradually brought back to 33°, the liminal temperature

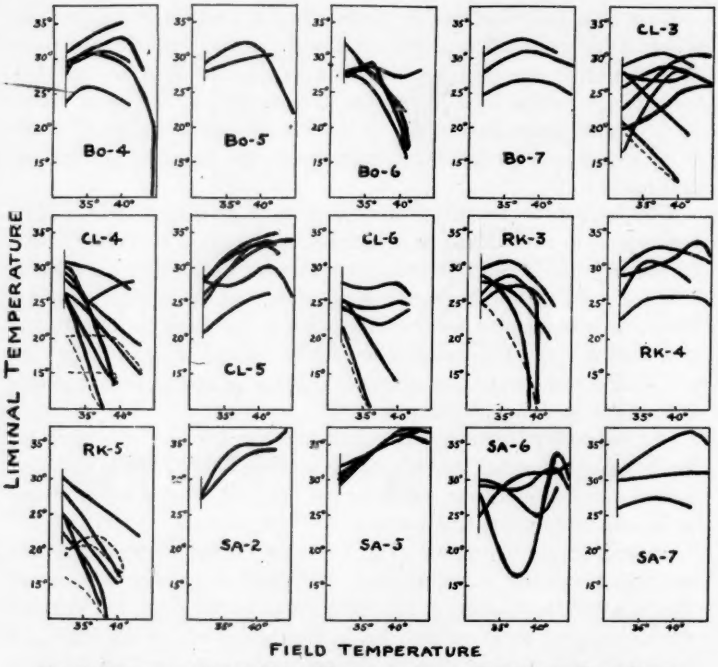

FIG. 7. EFFECTS OF RAISED FIELD-TEMPERATURE ON DIFFERENT COLD SPOTS

climbs again practically to its original value, as is shown by the dotted lines in Fig. 5 D and Fig. 7. This indicates that the depression is specific to the increase in field-temperature and cannot be ascribed merely to general fatigue. Apparently the downward trend starts as soon as the field becomes a degree or two warmer than normal, and continues without any sign of a critical level, such as was found in the case of lowered field-temperature.

Fig. 7 shows the effect of increased field-temperatures in a number of cold spots. Every curve in this figure is derived in a manner similar to those

shown in detail in Fig. 5. Notice that the general type of reaction seems fairly characteristic of the spot itself. For instance, *Bo*-6, *Cl*-4, *Rk*-3, and *Rk*-5 consistently show the third type of reaction, *i.e.* depression. *Cl*-5, *Sa*-2, and *Sa*-5 are rather consistent in showing the first type of reaction, *i.e.* elevation. Marked inconsistency, however, is found in spot *Cl*-3, where each of the three types of reaction was observed, several times apiece, on different occasions. It will be noticed that *Cl*-3 was also highly variable in the levels of its normal limen and in its reactions to low temperatures.

It is interesting to note that ordinarily the *O* never felt any temperature sensation from the field disk, although the rise was often as rapid as 1° per min. Above 45° prickling sensations usually set in, which interfered seriously with temperature discrimination. This accounts for the absence of systematic determinations above this level.

Discussion

(1) A working hypothesis. During the course of this investigation, it was found helpful to postulate a mechanism which would account for the observed phenomena and act as a guide for further research. Thus the following working hypothesis was found fruitful.

(a) Depression and restoration of the liminal temperature suggest as a basic mechanism a reversible reaction: A⇄B. This reaction proceeds rapidly in the direction A→B (depression) under the influence of a low temperature; and more slowly in the direction B→A (restoration) upon removal of the low temperature. The latter reaction can be speeded up by the application of a higher temperature (*i.e.* from 40° to 46°). This is not necessarily a chemical reaction, although it is convenient to think of it as such, because of its resemblance to the reactions of visual purple. Thus the bleached state of visual purple (low sensitivity) corresponds to the state B of our hypothetical cold receptor mechanism, and the purple state (high sensitivity) to state A. In both instances, the reaction proceeds rapidly in one direction and more slowly in the other. Whether the restoration of visual purple can be speeded up in any way is not known (to parallel the rapid restoration of B→A by high temperatures).

(b) The liminal temperature depends upon the state of balance existent in A⇄B at the moment. The further the reaction has proceeded in the direction A→B, the lower the temperature must be for stimulation. (Similarly with visual purple: the further it has moved toward the bleached state, the stronger the light must be to stimulate.) From the standpoint of liminal temperature, then, adaptation involves primarily a shift in the equilibrium of A⇄B to some new level. (Likewise with visual purple, adaptation involves a similar change.)

(c) The sensation of cold results when the reaction moves in the direction B→A, at a rate faster than some necessary minimum. (At this point, we must drop the analogy to visual purple.) Within limits, the more rapid the change, the more intense would be the resulting cold experience. The duration of the sensation depends upon the duration of the change, the experience of cold ceasing when move-

ment in the direction A→B ceases. It is evident that the change might last longer than the actual contact of the stimulating object, thus accounting in part for after-sensations.

(d) Such a simple reversible reaction, however, does not account for the facts of *stability*. It will be recalled that a cold spot may show a relatively high initial liminal temperature and yet 'fade' rapidly under stimulation. This suggests the presence of a substance 'C' which is used up during the restorative process. Assume that in 'fading' spots, very little 'C' is present; whereas stable spots have an ample reserve of it. Then it becomes apparent why two cold spots with the same initial liminal temperatures may behave quite differently upon repeated just-liminal stimulation. The supply of 'C' is quickly used up in the weak spot and restoration is no longer possible. Since even the weakest spots eventually recover, it seems probable that 'C' is slowly renewed or regenerated, possibly through the agency of the blood stream.

(e) This A⇄B and C theory was developed on the basis of experiments with the field-temperature maintained at 33°. How well does it explain the results obtained when the field-temperature was varied? The effects of gradually lowering the field temperature seem to fit in quite smoothly. The initial change in liminal temperature is readily explainable as a shift in the balance of A⇄B. Then it is only necessary to assume that the lowered field temperature acts adversely upon 'C' as well, and markedly so below a certain critical level. The reserve of 'C' becomes so depleted that there is not enough of it to take care of the restorative action, and hence the liminal temperature plunges sharply downward.

(f) The varied effects of gradually raising the temperature of the field are more troublesome. High temperatures, according to the theory, should cause the reaction to move in the direction B→A, resulting in a higher liminal temperature. This is what occurs in the first type of reaction. But what of the other two types, where there is definitely a downward trend? These curves might be explained as *resultants* of opposed effects upon A⇄B and 'C'. Thus, under some circumstances, the liminal temperature might be expected to move downward, because a depleted supply of 'C' cannot fully restore the reaction A⇄B to its former equilibrium after each testing stimulation. To duplicate the curves empirically obtained, however, it is necessary to assume a nicety of balance in these opposed effects that is scarcely credible. Perhaps it is better to let the explanation of these effects wait upon further critical evidence.

(g) It will be noticed that no attempt has been made to account for paradoxical cold in terms of this theory. Originally, it was expected to assign paradoxical cold to a very rapid restoration of B→A, on the ground that *any* change in equilibrium might produce a sensation if it were rapid enough. None of the *O*s, however, experienced paradoxical cold under the conditions of this experiment. To assign the sensations actually reported (hot, warm, and non-thermal) to the restoration phase of B→A would involve a revision of the doctrine of specific energies, which is too radical a procedure to be justified by the present evidence. On the other hand, the data of this experiment present no justification for explaining paradoxical cold on such a basis.

(2) Problem of larger areas. One fact, however, is plainly evident from the results of this study. The thermal sensitivity of larger areas cannot be

explained as a simple arithmetical addition of individual temperature spots. If this were so, one should be able to step under a cold shower, feel a brief burst of cold, and all would be neutral. At least, that is the way a single cold spot appears to act.

Thus, if the current theory of punctiform thermal sensitivity is to be upheld, the cold spots must operate upon some 'united we stand, divided we fall' principle. Just how this could be, it is not safe to speculate upon at the present time, but certainly, bridging the gap between the effects in single spots and in larger areas presents a fascinating field for further research.

This study offers little support for Nafe's ingenious theory that warm and cold sensations are to be explained as kinesthetic patterns from dilating and contracting blood vessels.[25] It is almost incredible that blood vessels could be expected to exhibit such a complicated cycle of reactions. The assumption of some sort of a definite receptor mechanism (possibly a physiological rather than an anatomical entity) seems to be a much more fruitful hypothesis.

Summary

With a procedure which permitted pure thermal stimulation (touch excluded) of single cold spots, surrounded by a field of controllable temperature, the following effects were observed on the left forearms of 6 *Os*.

(1) When a single cold spot was stimulated continuously at a constant temperature, the cold sensation usually died out entirely in less than 4 sec.

(2) Following continuous or intermittent stimulation at low temperatures, there was a long-lasting depression of the liminal temperature. Even with just-liminal stimulation, some cold spots 'faded out' completely after a few stimulations at 30-sec. intervals.

(3) Rapid restoration of the liminal temperature could be effected by one minute's contact with high temperatures, but such recovery was apt to be unstable when the depression had been extreme. Rest periods of 2–5 min. also brought about very unstable recovery.

(4) Under the conditions of the experiment, paradoxical cold was not experienced by any of the 6 *Os*, with stimulation at high temperatures. Instead, there were frequent reports of 'hot' and 'warm' when the cold spot was stimulated with temperatures about 45°.

(5) The reaction-time to stimulation of a single cold spot varied from *O* to *O*, but seemed to bear no direct relation to the stimulating tempera-

[25] J. P. Nafe, The pressure, pain and temperature senses, in Murchison's *Handbook of General Experimental Psychology*, 1934, 1054-1058.

ture. The reaction-times were considerably longer than those reported in the literature from stimulating of larger areas.

(6) When the temperature of the field surrounding the cold spot was gradually reduced, the liminal temperature held its own relatively until the field reached a certain critical temperature (from 28° to 22°). Then the liminal temperature dropped sharply until the spot would no longer respond even to 10°.

(7) When the temperature of the surrounding field was gradually raised, three different effects were observed: (a) the liminal temperature rose to a certain maximum; (b) there was an initial rise followed by a fall to about the original level; (c) there was no rise whatever, but the liminal temperature fell steadily as the field-temperature rose, and rose again if the field temperature was brought back to normal.

(8) To account for the elevation and depression of the liminal temperature, a reversible reaction $A \rightleftarrows B$ is suggested, which bears some resemblance to the reversible reaction of visual purple. To cover the facts of stability and instability, an accessory substance 'C' is postulated. This is present in different spots in varying amounts, and is used up in bringing about the restoration phase $B \rightarrow A$. Such a mechanism explains reasonably well the results secured in this study of single cold spots. Bridging the gap between these effects and those traditionally secured with larger areas is left for further research.

'FLUCTUATIONS OF ATTENTION' WITH WEAK TACTUAL STIMULI: A STUDY IN PERCEIVING

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The topic of attention has had a long and interesting history in the field of psychology. The laws, conditions, and characteristics of attention have received intensive treatment at the hands of the systematist, and the experimental literature abounds with crucial experiments on one aspect or another of this field. One of the most fruitful and at the same time most controversial aspects has been the problem of 'fluctuation.'

The method of minimal stimuli, regarded as an achievement of the first order when it was introduced, has been used in the study of the 'fluctuation of attention' because it was thought to be much easier to report upon the presence or absence of an experience than to report upon a change in or a 'fluctuation' of the degree or level of attention given to the experience. The specific approach to this problem has usually been to present a weak stimulus to the *O*, which requires maximal attention to perceive it, with instructions to report its disappearance and reappearance.

The results of these investigations have shown that perceiving is intermittent; *i.e.* the light was reported as seen, not seen, seen, etc.; the tone as heard, not heard, heard, etc.; the 'touch,' taste, odor, as experienced, not experienced, etc. These results have been interpreted to mean that attention fluctuates. Before this interpretation can be accepted, three points must, it was early realized, be unquestionably established. (1) The fluctuations must be observed in *all* sense departments. If they are lacking in any one, the phenomenon cannot be ascribed to attention. If they occur in all of the different sense departments, the fluctuations cannot, however, be definitely ascribed to attention until (2) they are demonstrated to be due to central and not to peripheral factors. If demonstrated to be central, there still remains (3) the necessity of establishing the final fact that they are attentional factors. Studies dealing with 'fluctuation' have been directed upon the first two of these aspects—there being no point in considering the third until the first two have been positively and definitely established.

Experimental results that have been obtained in answer to the first two of these problems have been equivocal. Some experimenters have reported

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fluctuations in all sense departments, while others have found fluctuations in only some of the departments. Furthermore, the experimenters reporting fluctuations have not always agreed whether the conditions responsible were central or peripheral.

A determination of the extent to which the fluctuations are dependent upon peripheral factors, or upon the structure and functioning of end-organs can best be accomplished by comparative studies in the various sensory fields. For this approach, the structure and function of the visual, auditory, and tactual mechanisms are ideally varied.

The visual organ involves a retinal area which shows adaptation and a complicated musculature for accommodation and convergence which is subject to fatigue and tremors. A small and weak light can only be seen if the lens is properly accommodated and the light waves fall on a non-adapted part of the retina. A minimal light may, therefore, disappear by way of adaptation and reappear when a slight shift in the muscular tension brings the light to a new retinal area. In the ear we have accessory musculature (the tensor tympani and the stapedius muscles) which undoubtedly fatigue, but the phenomenon of sensory adaptation is wanting. The tactual end-organs adapt, but they have no muscles to complicate their functioning. Vision thus possesses both of the factors that may make for the 'fluctuation' of minimal stimuli (sensory adaptation and muscles that fatigue and tremor); audition possesses one of them (the muscular), and touch the other (sensory adaptation).

Guilford found that 'fluctuations' with weak visual stimuli were due to a central factor as well as to peripheral ones.[1] The central factor is "fatigue or inhibition, a reduced readiness to respond to the same stimulation,"[2] which is local, being limited to a small part of the optic center, very instable, and of the all-or-none type. The peripheral factors are of three kinds: (1) eye-tremors which favor visibility; (2) local retinal adaptation, winks, and refixations which favor the disappearance of a weak light; and (3) winks, refixations, and other factors which favor the return of a weak light that has disappeared. These results led him to conclude that "fluctuation with liminal stimuli is a phenomenon which is connected with all liminal stimuli,"[3] and that the phenomena, because peripheral as well as central factors are involved, cannot be designated as the "fluctuation of attention."

The purpose of the present study is to extend the investigation into the tactual field in order to discover the part the end-organ plays in determin-

[1] J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this JOURNAL, 38, 1927, 534-583.
[2] *Idem*, 577.
[3] *Idem*, 583.

ing this intermittency or 'fluctuation' of perceiving.[4] At a later date the problem will be carried into the auditory field.

Historical Setting

Although the 'fluctuation' of minimal visual sensations was observed and reported by Hume,[5] the first systematic study of fluctuations of minimal experiences was made by Urbantschitsch.[6] He used auditory stimuli in his pioneering work, but later extended his investigations into other sensory fields. He obtained fluctuations of pain, warmth, cold, taste, smell, vision, and touch.[7] His tactual stimuli were the points of a compass, of which he sometimes used but one. He obtained fluctuations of two kinds. Tactual experience, he found, might be (1) present or absent, and (2) clear or unclear. When both points of the compass were used, he found that they fluctuated independently as well as synchronously.

Lange,[8] the second to investigate this problem, used tactual among other kinds of stimuli. A current from a Du Bois-Reymond induction coil was adjusted to give weak shocks. One pole was held in the palm of the moistened hand while the other was placed in a vessel of lukewarm water. The circuit was completed by immersing a finger of the free hand into this vessel at a definite and constant depth. Fluctuations were reported.

Lehmann,[9] the third investigator to study fluctuations of tactual stimuli, also used an induction coil. One electrode was placed on the lower arm, and the other in a porcelain dish containing water of body temperature. A glass rest enabled *O* to hold a finger in the water at a definite and constant depth. Fluctuations were obtained which averaged 4.1 sec. in length. Since this was the length of the breathing rhythm, Lehmann correlated them, and concluded that respiration was indirectly responsible for the rhythm of the tactual fluctuations.

Hylan was the next to experiment upon fluctuations in the cutaneous field.[10] He stimulated the back of the hand with a rubber tube 1 mm. in diam., through which cold water flowed. Hylan found independent fluctuations of cold and pressure, and a relationship between the strength of the pressure and the fluctuations—the stronger the pressure, the longer the periods when pressure was not experienced. He concluded that this "may be explained, perhaps, by the fact that an unusual pressure keeps the blood away from the place of contact and brings on numbness, but it is difficult to see why that numbness disappears again."[11]

Wiersma applied tactual stimuli—weighted cork wafers 2 cm. in diam.—to the

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[4] This study was made in the Psychological Laboratory of Cornell University under the direction of Professor Karl M. Dallenbach.

[5] David Hume, *Treatise on Human Nature,* 1739, ed. by L. A. Selby-Bigge, 1896, 41 f.

[6] Victor Urbantschitsch, Über eine Eigenthümlichkeit der Schallempfindungen geringer Intensität, *Centralbl. f. d. med. Wiss.,* 1875, 625-628.

[7] Victor Urbantschitsch, Über subjective Schwankungen der Intensität acustischer Empfindungen, *Arch. f. d. ges. Physiol.,* 27, 1882, 436-453.

[8] Nicolai Lange, Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, *Phil. Stud.,* 4, 1888, 390-422.

[9] Alfred Lehmann, Über die Beziehung zwischen Athmung und Aufmerksamkeit, *Phil. Stud.,* 9, 1894, 66-95.

[10] J. B. Hylan, Fluctuations of the attention, *Psychol. Rev.,* 3, 1896, 56-63.

[11] *Ibid.,* 62.

back of his *Os'* hands.[12] The stimuli, 6 in number, varied in weight by 3-gm. steps, from 7.4 to 22.4 gm. The lightest was barely perceptible, and the heaviest almost always perceptible. Wiersma not only obtained fluctuations, but also established a relationship (1) between the stimulus-value and length of fluctuation, and (2) between the stimulus-value and the total time of perception. The heavier the weight the longer the individual perception, and the greater the total time perceived.

Ferree,[13] using smooth cork wafers, and electrical stimulation of the tongue, reported that he was unable to obtain fluctuations. He failed to mention, however, whether the experiences aroused by these stimuli were continuously present during the period of the experiment, or only momentarily present.

Geissler[14] thought to repeat Wiersma's and Ferree's work. He reproduced Wiersma's conditions with the exception that the weights were 1 instead of 2 cm. in diam. Though he used Wiersma's weights, the reduction of the diameter raised them to the supraliminal range, and perhaps as a consequence none of his 7 *Os* reported fluctuations. Geissler then constructed a lighter set of weights of the same diameter (1 cm.), which ranged from 0.5 to 6 gm. From the results with these weights, Geissler concluded that "a liminal areal stimulus applied to the skin after about a minute's time loses its original character, and either disappears altogether after the lapse of another minute or two, or persists as a simple, indefinite, vague, and unpleasant irritation."[15] In repeating Ferree's experiment—electrical stimulation of the tongue—Geissler found that the experience did not disappear during the 2-min. interval of the experiment.

All of the experimental work on the fluctuation of tactual experiences has been performed on normal tissues with normal end-organs. Various kinds of stimuli have been employed, often without regard to their intensity or fitness, and sometimes without considering the nature of the tissues stimulated. No experimenter used point-stimuli on only one end-organ, but probably stimulated a great many. Urbantschitsch was the only experimenter who used point stimuli—viz. compass points—and he secured fluctuations. Lange and Lehmann secured fluctuations by stimulating with the current from an induction coil. Ferree and Geissler used the same form of stimuli and failed to secure fluctuations. A variety of factors might account for this difference of result. Perhaps they used currents of different strengths. There may have been a difference in the rapidity of the individually induced currents, or of the constancy of the skin resistances of the *Os*. Slight movements of the fingers of Lange's and Lehmann's *Os* might easily have produced fluctuations. Furthermore, we have no idea what constituted a fluctuation for any of the *Os*. Certainly more than simple tactual or pressure experiences were aroused by electrical stimulation. Finally, we know very little about the exact stimulating effect of such a current in the skin and subcutaneous tissues.

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[12] E. Wiersma, Untersuchungen über die sogenannten Aufmerksamkeitsschwankungen, *Zsch. f. Psychol.*, 26, 1901, 168-200.

[13] C. E. Ferree, An experimental examination of the phenomenon usually attributed to the fluctuation of attention, this JOURNAL, 17, 1906, 81-120.

[14] L. H. Geissler, Fluctuations of attention to cutaneous stimuli, *ibid.*, 18, 1907, 309-321.

[15] *Ibid.*, 318.

Hylan, Wiersma, and Geissler used areal stimuli for arousing pressure experiences. Hylan and Wiersma reported fluctuations, and Geissler secured some under certain conditions and attitudes of his *O*s. Wiersma secured some adaptational effects, and Geissler found adaptation the outstanding phenomenon. No experimenter computed limens for his *O*s. All supposedly used weak stimuli, approximating the liminal value, but only Wiersma and Geissler gave the actual stimulus-values employed. Wiersma's and Geissler's results indicate that they used stimulus-values above, below and of about liminal value. They limited the movements of the arms of their *O*s by using plaster casts, but this is only one precaution necessary in this type of work. Furthermore, neither mentions that movements occurred or that he checked them in any way. A further difficulty that they must have discovered but failed to mention, was the shifting, very slight perhaps, of the position of the weight during the course of the stimulation. This would, of course, stimulate new end-organs and if adaptation had occurred previously, tactual experience would again occur, to adapt, etc.,—to give what has been called 'fluctuations.' Geissler, in attempting to repeat Wiersma's experiment, used stimuli of 1-cm. diam. instead of the 2-cm. which Wiersma used. Naturally the weights were supraliminal for all of his *O*s. He then made a lighter set, and evidently succeeded in eliminating enough movement to prevent fluctuations, even though he stimulated a portion of the back of the hand directly over numerous blood vessels, ligaments and fascia—the same region Wiersma used.

The results of the studies on the skin are conflicting. It is still an unsettled question whether the experiences aroused by weak tactual stimuli fluctuate. None of the proponents of the point of view that fluctuations do occur gives a statement of the necessary and sufficient conditions; nor does any give an exact definition in liminal terms of the intensity of his stimuli. We determined, therefore, to repeat this study, but to repeat it with stimuli whose intensive relations to the limen were definitely known. We worked with punctiform and areal stimuli on cutaneous tissues and with punctiform stimuli on subcutaneous tissues.

I. Cutaneous Tissue: (A) Punctiform Stimulation

(a) Preliminary experiments. In order to determine the stimulus-values required for the punctiform stimulation of our *C*s, we conducted a series of preliminary exploratory experiments with a large number of hair-esthesiometers.

Apparatus. The esthesiometers were made from human hairs with handles of drawn glass tubing about 4 cm. long and 3 mm. in diam. The hairs were inserted in the tubing and fastened to the drawn end by sealing wax. The free ends of the hairs were then cut off squarely with a sharp razor and the hairs numbered for purposes of identification. The bending value of each R-hair (as we shall call the stimulus-hairs) was determined on a chemical balance, and the diameters of the stimulating end were measured under a microscope. The tension value of the hair was then determined by the Von Frey formula—T = gm./mm., in which gm. is the bending value of the hair in grams and mm. is the average radius of the

hair in millimeters.[16] By using hairs of various lengths and diameters a wide range of stimulus-values was obtained. These R-hairs were presented by means of a kinohapt.[17]

Procedure. The experiments were made on the dorsal surface of *O*'s left forearm. In order to eliminate involuntary movements and to insure constancy of the experimental conditions plaster casts were made for every *O*.[18]

A circular area on the lateral surface of the dorsal forearm, about 2 cm. in diam., and from 4 to 6 cm. from the elbow, was shaved. One hair was, however, left intact in the center of the area. This hair was marked by a tatooed point placed 0.5 mm. lateral to the hair,[19] and served thereafter as a landmark for purposes of stimulation.

At the beginning of each experimental period *O* seated himself in the armchair, placed his left arm in the plaster cast and was then given the following instructions to read.

After 'Ready—Now' you will be stimulated tactually. Report 'yes' if tactual experience follows, and 'no' if no tactual experience occurs. Try to maintain a relaxed and passive attitude throughout the experimental period.

E then introduced an R-hair into the kinohapt, adjusting it so the stimulating end was *just above* the surface of the skin at a point about 0.5 mm. windward of the emergence of the arm-hair from the skin. The signal 'Ready—Now' was given, followed by the closing of the circuit of the kinohapt. A Leitz binocular microscope of 10.5 magnification was used by *E* in observing each presentation.

Observers. The *O*s who took part in this experiment were: Dr. Ellis Freeman (*F*), instructor in psychology; Mr. A. D. Glanville (*G*), scholar in psychology; and Mr. E. H. Kemp (*K*), graduate student in psychology. All *O*s had had previous experience in psychological observation.

After a long practice series during which there was a gradual decrease in the intensity of the stimulation necessary for the experiencing of pressure, there was a period of stabilization of report. The liminal determination for each *O* was then made on the basis of the data secured from the presentation of each of 5 stimulus-values in 4 series of 15 presentations each. The arranged haphazard order of presentation was used. In the case of *K* the irregularity of report led to the selection of another hair on the arm as a focal point of stimulation, with a consequent increase in consistency of report. No presentation in which there was any movement of the arm or failure of the hair to stimulate properly was used in the liminal determination. Every sixth experiment was used as a check, *i.e.* the signal 'Ready—Now' was not followed by stimulation.

The tension values and data from which they were computed, are shown in Table I for the 5 stimulus-values selected for each *O*. In every case one value was approximately liminal, two were above and two below. There is a definite rela-

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[16] Max von Frey, *Untersuchungen über die Sinnesfunctionen der menschlichen Haut*, 1, 1896, 229 ff.

[17] K. M. Dallenbach, A kinohapt, this JOURNAL, 34, 1923, 90-92.

[18] For a discussion of sources of error to be avoided in punctiform stimulation of end-organs of the skin see K. M. Dallenbach, The temperature spots and end-organs, *ibid.*, 39, 1927, 409-413.

[19] For the method employed see K. M. Dallenbach, A method of marking the skin, *ibid.*, 43, 1931, 287.

tionship, for every *O*, between the intensity of the stimulus and the percentage of times experience was aroused. The liminal values as computed by the constant method were: *F*, 0.6742, h 3.3744; *G*, 0.9857, h 1.5191; *K*, 0.6243, h 3.6620.

(b) Main experiment. Having determined the limens for our *O*s for punctiform stimulation, our next problem was to discover whether the tactual experiences aroused by liminal and near liminal stimuli fluctuated; and if so to determine whether their rate of fluctuation varied with their nearness or remoteness from the limen.

TABLE I

PERCENTAGE OF TIMES THAT THE 5 STIMULUS-VALUES SELECTED FOR EVERY *O* AROUSED EXPERIENCES

R-hair No.	Bending value (gm.)	Av. rad. (mm.)	Tens. val. (gm./mm.)	Observers		
				F	*G*	*K*
90	0.018	0.0377	0.477	17%		18%
110	0.025	0.0433	0.577	33%	17%	47%
92	0.025	0.0360	0.694	53%		67%
91	0.030	0.0405	0.741	65%		
107	0.035	0.0433	0.808	72%	35%	77%
97	0.035	0.0386	0.906		48%	95%
95	0.040	0.0396	1.137		63%	
108	0.060	0.0399	1.503		85%	

Apparatus. The apparatus used in the preceding part of the experiment was employed. In addition a reaction-key, wired in circuit with a pen of a Renshaw polygraph,[20] was provided *O*. Another pen indicated the beginning of the stimulation. A Zimmerman time-clock set to mark at 1 sec. intervals was connected with a third pen of the polygraph. Closing one switch started the polygraph and the time-clock; closing the other switch stimulated *O* and simultaneously recorded the time of stimulation. The record resulting from the depression or release of the telegraph key of *O* indicated the presence or absence of tactual experience.

Procedure. *O* seated himself in the armchair, placed his left arm in the plaster cast, and his right index finger on the telegraph key. He was then given the following written instructions.

After 'Ready—Now' you will be stimulated tactually. Depress the key as soon as tactual experience occurs. Release it when there is no tactual experience. Attend until the signal 'Stop' is given. Maintain a relaxed and passive attitude throughout the experimental period.

During the course of a single experimental period, *E* presented each of the 5 R-hairs selected 4 times in a planned haphazard order—three times for a period of 1 min. and once for a period of 2 min. Five series were run, making 20 stimulations for each hair or 100 for each *O*. The polygraph was isolated in a distant room and was run at slow speed.

[20] For a description see Samuel Renshaw and A. P. Weiss, The recorder unit, this JOURNAL, 37, 1926, 266 f.

Results. Ordinarily there was a short latent period following stimulation, succeeded by perception of pressure in proportion to the stimulus-value. This pressure lasted for a short time, and then disappeared. Table II shows the average latent period and average duration of experience for every *O* for the 5 stimulus-values selected. The latent period and duration of experience vary with the stimulus-value: the more intense the stimulus, the shorter the latent period; and the more intense the stimulus, the longer the duration of pressure.

TABLE II

AVERAGE LATENT PERIOD AND AVERAGE DURATION OF EXPERIENCE (IN SEC.) FOR THE 5 STIMULUS-VALUES SELECTED FOR EVERY *O*

O	Limen	R-hair No.	Tens. Val. (gm./mm.)	Latent period		Duration	
				Av.	A.D.	Av.	A.D.
		90	0.477	0.70	0.20	1.63	0.24
		110	0.577	0.67	0.37	3.63	1.34
F	0.674	92	0.694	0.50	0.22	3.32	1.94
		91	0.741	0.69	0.29	2.44	1.07
		107	0.808	0.41	0.09	3.44	2.03
		110	0.577	1.13	0.35	0.36	0.22
		107	0.808	0.64	0.38	1.22	0.81
G	0.986	97	0.906	0.96	0.51	1.08	0.47
		95	1.137	0.94	0.47	1.24	0.50
		108	1.503	0.64	0.28	1.57	0.72
		90	0.477	1.43	0.67	3.34	2.27
		110	0.577	1.66	0.74	8.26	3.03
K	0.624	92	0.694	1.52	0.57	3.95	3.20
		107	0.808	1.13	0.27	5.18	2.57
		97	0.906	1.26	0.42	6.82	3.51

The average latent periods for every *O* are much longer than the muscular reaction-times ordinarily given for pressure stimuli. This difference is probably due to the weak stimulus-values employed. *K* shows an unusually long latent time in comparison with *F* and *G*. Adaptation to these stimuli is extremely rapid, occurring in almost every case under 10 sec. *K* also differs in this respect from *F* and *G*, his experiences lasting much longer.

F and *K* reported no fluctuations. *G* showed 4 cases of return of experience after it had once disappeared. One case was with R-hair No. 107, tension value of 0.808. There was experience for 2.2 sec., disappearance for 0.3 sec., experience for 1 sec., followed by permanent disappearance. Two instances were with R-hair No. 97, tension value of 0.906. The first of these showed experience for 1.4 sec., disappearance for 3.6 sec., experience for 1.6 sec., followed by permanent disappearance. In this case *E* observed a muscle twitch underlying the stimulated spot at the time of

reappearance of experience. The second instance with R-hair No. 97 gave experience for 0.5 sec., disappearance for 1.6 sec., experience for 1.2 sec., which was followed by permanent disappearance. The fourth case was with R-hair No. 95, tension value of 1.137. Tactual experience lasted for 1.3 sec., disappeared for 1.7 sec., returned for 0.3 sec., to disappear permanently. *E* again observed movement of the arm tissues.

In view of the results obtained here we feel justified in concluding that weak cutaneous stimuli, which are near the limen, do not yield 'fluctuations,' but become adapted very rapidly.

(B) Areal Stimulation

Our purpose in this part of the experiment was to discover whether the tactual experiences aroused by weak areal stimuli would fluctuate. In the previous part of the experiment we stimulated a single end-organ, and found rapid adaptation to pressure and no fluctuations which may be ascribed to attention. We wanted to discover the effect of simultaneous stimulation of two or more pressure end-organs upon the temporal course of perceiving of pressure.

Apparatus. Areal stimulators were prepared as follows. Horse hairs of various lengths and diameters were fixed in glass tubes numbered for identification. To the distal end of each hair was fastened, by a small globule of sealing wax, a circular cork wafer 1.5 mm. thick and 4 mm. in diam. The bending value of the hairs in grams was then determined on a chemical balance, and the tension value computed for every stimulator. They were applied mechanically by means of the kinohapt used in the first part of the study.

Procedure. The same area of the skin (the upper dorsal forearm) was used as before. This area was selected because it presented a flat even surface upon which the cork wafers could be pressed. Three spots, which delimited the area to be stimulated, were tatooed in the skin. The area was shaved at every experimental period so as to avoid the experiences aroused by the stimulation of the hairs. Every stimulation was observed through a binocular microscope. Great care was taken to see that the lower surface of the cork wafer was parallel to and just above the surface of the skin at the moment before stimulation. Experiments were discarded in which the surface of the cork wafer did not hit the skin evenly. The method of procedure was the same as that of the punctiform series.

Observers. The *O*s in this part of the experiment were Mr. E. H. Kemp (*K*) and Mr. A. D. Glanville (*G*), both of whom had served in the punctiform series of experiments.

Of the stimulators prepared, 5 were selected on the basis of the preliminary experiments. Table III shows the tension values of those selected together with the percentage of times that they aroused experiences in each *O*. A definite relationship exists between tension value and the percentage of times that tactual experiences are aroused.

TABLE III

PERCENTAGE OF TIMES THAT THE 5 STIMULUS-VALUES SELECTED FOR EVERY *O* AROUSED EXPERIENCES

R-No.	Bending value (gm.)	Radius (mm.)	Tens. val. (gm./mm.)	Observers	
				G	*K*
103	0.05	2.0	0.025	15%	22%
111	0.07	2.0	0.035	40%	38%
102	0.08	2.0	0.040	57%	47%
112	0.09	2.0	0.045	70%	60%
114	0.10	2.0	0.050	83%	78%

None of the stimulus-values selected for the *O*s was liminal, but one was near that value, and two were above and two below it. The limens, as computed by the constant method were: *G*, 0.038, h 56.24; *K*, 0.039, h 42.02.

Results. The course of experience with areal stimuli was very similar to that aroused by punctiform stimuli. Ordinarily, after a short latent period a weak pressure was experienced, which persisted for a short time and then disappeared.

Table IV shows a definite shortening of the average latent period and an increase in the average duration of experience with an increase of stimulus-value for *G*. *K* does not show this relationship. As with punctiform stimuli, *K*'s average latent period and duration of experience are longer than *G*'s. We have no explanation for this difference. Adaptation is very rapid for both *O*s, usually less than 1 sec. for *G*, and less than 10 sec. for *K*.

TABLE IV

AVERAGE LATENT PERIOD AND AVERAGE DURATION OF EXPERIENCE IN SEC. FOR 5 STIMULUS-VALUES SELECTED FOR EVERY *O*

O	Limen	R-No.	Tens. val. (gm./mm.)	Latent period		Duration	
				Av.	A.D.	Av.	A.D.
		103	0.025	0.27	0.09	0.23	0.09
		111	0.035	0.23	0.11	0.50	0.33
G	0.038	102	0.040	0.22	0.08	0.47	0.30
		112	0.045	0.25	0.13	0.46	0.14
		114	0.050	0.15	0.06	0.97	0.37
		103	0.025	0.63	0.11	7.40	0.76
		111	0.035	0.49	0.05	9.29	1.27
K	0.039	102	0.040	0.56	0.09	9.38	4.85
		112	0.045	0.49	0.09	8.73	3.23
		114	0.050	0.54	0.11	8.97	3.82

G reported no fluctuations, and *K* reported only two, both of which occurred with the strongest stimulus (R-No. 114), the one with tension value of 0.050. In the first of these there was tactual experience for 8.1

sec., disappearance for 42.8 sec., experience for 0.8 sec., followed by permanent disappearance. The second case showed tactual experience for 2.8 sec., disappearance for 7.1 sec., experience for 6.7 sec., followed by disappearance for the remainder of the period. In neither case did *E* detect movement of *O*'s arm, though it was continually observed during the time of stimulation through the microscope.

The results from this experiment with areal stimuli agree perfectly with those obtained with punctiform stimuli. In view of these results we feel justified in concluding that weak areal stimuli, near the limen, do not give fluctuations, but become adapted very rapidly.

II. Subcutaneous Tissue: Punctiform Stimulation

The experiments upon the subcutaneous tissues were of two kinds: (*1*) those made upon etherized skin, in which the cutaneous end-organs were anesthetized by spraying with ether; and (2) those made upon scar tissue, in which the cutaneous end-organs were lacking.

(1) Etherization of the skin. In the first experiments upon the subcutaneous tissues we etherized the skin.

Apparatus and procedure. We chose for experimental purposes the same area (the dorsal forearm) that was used in the previous experiments. The same *O*s who served in the punctiform experiments upon the cutaneous tissue observed in these experiments; Dr. Ellis Freeman (*F*), Mr. A. D. Glanville (*G*), and Mr. E. H. Kemp (*K*). Similar stimulators, but made of horse hair instead of human hair, were used. They were mounted in glass tubing, numbered and calibrated as before. They were presented in the kinohapt.

Results. Our experimentation was unsuccessful for the following reasons: (a) the odor of the ether was very distracting—sometimes nauseating; and (b) the complex of experiences—principally cold and sting—produced by the ether, masked the pressure experiences aroused from the subcutaneous tissues. These factors made liminal determinations and a study of the temporal course of pressure experiences aroused in the subcutaneous tissue very difficult. We consequently abandoned the procedure.

(2) Scar tissue. Since scars do not contain the pressure end-organs usually found in cutaneous tissue we decided to stimulate the subcutaneous pressure end-organs by applying our stimulators to scar tissue.

Observers. Two *O*s, Professor K. M. Dallenbach (*D*), and Mr. W. H. McCann (*M*), scholar in psychology, served in this experiment. *D*'s scar was irregular in shape, about 3 cm. long and 2.5 cm. wide. It was on the medial and anterior surface of the lower left leg, about 8 in. below the knee. *M*'s scar was almost perfectly circular in shape, 2 cm. in diam., on the lateral surface of the upper left leg, about two-thirds of the distance from the knee to the thigh.

Apparatus and procedure. We again used horse hairs, mounted in glass tubes,

numbered and calibrated, as stimulus-objects. Since, however, the scars were located in places where it was difficult to use the kinohapt, *E* decided to stimulate by hand, resting the wrist on a wooden support during the course of the stimulation. The hairs surrounding the scars were clipped to permit easy access to the scar tissues. *D* sat in a chair, with his left leg supported comfortably by a small table of the proper height. *M* also sat in a chair, with legs extended comfortably. Both *O*s were given the following instructions to read at the beginning of each experimental period.

At 'Ready—Now' you will be stimulated with a blunt point. Report the experiences immediately aroused, if any, and then describe them during their temporal course. Relax and remain as motionless as possible during the course of the experiment. Close your eyes at 'Ready—Now,' and do not open them until told to do so.

E stimulated *O* near the center of the scar area. Every stimulation lasted for 50 sec. *E* then recorded the reports of *O* during the period of stimulation. A 4-min. rest period was introduced every 15 min. during the experimental period. The haphazard order of presentation of stimuli was used.

Results. The results from these experiments were used in computing the limens and also for the study of fluctuations. Table V shows the percentage

TABLE V

PERCENTAGE OF TIMES THAT THE STIMULUS-VALUES SELECTED FOR EACH *O* AROUSED EXPERIENCES

R-hair No.	Av. radius (mm.)	Bending value (gm.)	Tens. val. (gm./mm.)	Observers	
				D	M
80	0.0855	0.39	4.62	8%*	
35	0.1070	0.74	6.92	18%	
42	0.1200	1.06	8.83	40%	6%
39	0.0845	1.28	15.37	54%	18%
67	0.1295	1.47	11.36	68%	22%
19	0.1190	1.68	14.11	74%	38%
76	0.1105	2.00	18.08	80%	36%
63	0.1230	3.58	29.15	96%*	76%

* Based on 25 presentations; others on 50 presentations.

of times that the stimulus-values selected for each *O* aroused experiences. With the exception of R-hair No. 76 for *M*, there is a definite relationship between intensity of stimulus and percentage of times experience is aroused.[21] The limens for our *O*s computed by the constant method (based on bending value) were: *D* (computed on R-hairs No. 35-76) 1.258 gm., h 0.998; *M*, 2.501, h 0.5183.

Stimulation was ordinarily followed by a short latent period, after which experience occurred in proportion to the strength of the stimulus. Experi-

[21] We found that the Von Frey formula for tension value does not hold for stimulation of subcutaneous tissues. The bending value of the hair is a better measure on scar tissue. Evidence for this will be presented in a later paper.

ence usually lasted for a short time—occasionally changed in nature—and then disappeared for the remainder of the period of stimulation. Table VI shows the percentage of times that the different stimulus-values (1) adapted, (2) failed to adapt, (3) fluctuated, and (4) failed to arouse experience. For *D*, 2.6% and for *M*, 2% of the stimulations gave rise to 'in and out' fluctuations, *i.e.* the experience disappeared, to reappear, etc. In some cases experience then continued until the stimulus was removed, and in others it disappeared permanently. *D* also showed 1.1% of 'Intensive' fluctuations, *i.e.* a report during the period of stimulation of 'weaker,' 'stronger,' etc., 'less intense,' 'more intense,' etc. For *D* 42.9% of the stimulations led to experience which disappeared before the end of the 50-sec. period of stimulation, whereas only 9.7% of the stimulations led

TABLE VI

PERCENTAGE OF TIMES THAT THE DIFFERENT STIMULUS-VALUES ADAPTED, FAILED TO ADAPT FLUCTUATED, AND FAILED TO AROUSE EXPERIENCE

Experience	O	Bending Value of Stimulators (in gm.)								Av.
		0.39	0.74	1.06	1.28	1.47	1.68	2.00	3.58	
Adapted	*D*	8*	16	34	46	54	62	58	52*	42.9
	M	—	—	6	16	20	34	34	74	30.7
Failed to adapt	*D*	0*	0	4	6	8	12	20	36*	9.7
	M	—	—	0	0	0	0	0	0	0
Fluctuated:	*D*	0*	2	2	2	6	0	2	8*	2.6
'In and Out'	*M*	—	—	0	2	2	4	2	2	2.0
Fluctuated:	*D*	0*	0	0	2	4	2	0	0*	1.1
'Intensively'	*M*	—	—	0	0	0	0	0	0	0
None	*D*	92*	82	60	44	28	26	20	4*	43.7
	M	—	—	94	82	78	62	64	24	67.3

* Based on 25 presentations; others on 50 presentations.

to experience which remained for the period of stimulation. For *M*, 30.7% of the stimulations led to experience which disappeared before the end of the 50-sec. period of stimulation, and in no case did experience last the full 50 sec.

In view of the fact that it is impossible to present the stimuli for 50 sec. without some movement of the tissues stimulated in relation to the stimulator, we feel that our results warrant the conclusion that fluctuations of experience aroused by stimulating subcutaneous tissues with punctiform stimuli of approximately liminal values do not occur. The principal result, as with punctiform and areal stimulation of cutaneous tissues, is rapid adaptation.

CONCLUSIONS

We believe we have avoided the errors commonly made in experimental work with pressure end-organs using point and areal stimuli. On the basis of our results we draw the following conclusions.

(1) This is a study of the temporal course of pressure experiences aroused by stimulation of cutaneous tissues with point and areal stimuli, and of subcutaneous tissues with point stimuli—all of approximately liminal value.

(2) Adaptation to punctiform stimulation of cutaneous tissues is very rapid when only one end-organ is stimulated—10 sec. or less.

(3) Adaptation to areal stimulation of cutaneous tissues with an area 4 mm. in diam. is also very rapid—usually 10 sec. or less.

(4) Adaptation to punctiform stimulation of subcutaneous stimuli is somewhat slower, but usually occurs within a period of 50 sec.

(5) The rate of adaptation depends on the intensity of the stimulus—the smaller the stimulus-value the more rapid the adaptation.

(6) The length of the latent period following stimulation is a function of the stimulus-intensity—the larger the stimulus-value the shorter the latent period.

(7) The most striking phenomenon following the stimulation of cutaneous or subcutaneous pressure end-organs with point or areal stimuli of approximately liminal value is the rapid disappearance of the experience thus aroused. The few 'fluctuations' which do occur result from inadequate controls in experimenting.

(8) The disappearance of the tactual experience is due to the rapid adaptation of the receptors involved—and no amount of attention will bring back the experience. Therefore, the temporal course of experience is directly due to peripheral factors, eliminating the phenomenon from the field of attention.

DIFFERENTIAL GUSTATORY SENSITIVITY TO SALT

By ALFRED H. HOLWAY and LEO M. HURVICH, Harvard University

This paper attempts to determine experimentally the functional relation between differential sensitivity to salt and the concentration of the stimulus-solution, and to study the spatial and temporal qualitative patterns aroused by a simple saline stimulus.

There have been four phychophysical experiments concerned with the course of differential sensitivity in taste. Three of them have used salt as a stimulus. Keppler,[1] Camerer,[2] and Fodor and Happisch,[3] employing a standard and a comparison stimulus, obtained limens at various points over a limited range of stimulus-concentrations. The essential interests of these writers were two: (1) to determine differences in the size of limens for salt as a function of 'zunehmende Differenz' and 'abnehmende Differenz' of the comparison stimuli, and (2) to test Weber's law for taste. None of these investigators was primarily interested in differential sensitivity to salt *per se*, with the manner in which differential sensitivity varies as a function of the stimulus throughout the intensitive range.[4]

Material. Salt is a relatively simple, familiar, and distinct quality.[5] Reaction-times for the four fundamental taste-qualities indicate that the time is shortest in the case of salt. Moreover, the discrepancy between simple reaction-time and discrimination-time is least for salt, a fact which would seem to indicate that salt, when perceived, is more clear and less ambiguous than the other qualities.[6]

Sodium Chloride (NaCl),[7] which alone of all the inorganic salts tastes purely *salt*,[8] was used as the stimulus in this experiment. (Oehrwall has shown that many

* Accepted for publication August 31, 1935.

[1] F. Keppler, Das Unterscheidungsvermögen des Geschmackssinnes für Konzentrationsdifferenzen der Schmeckbaren Körper, *Arch. f. d. ges. Physiol.*, 2, 1869, 449.

[2] W. Camerer, Die Methode u.s.w., *Zsch. f. Biol.*, 21, 1885, 570. Cited by E. von Skramlik, *Handbuch der Physiologie der niedern Sinne*, 1, 1926, 504.

[3] K. Fodor and L. Happisch, Über die Verschiedenheit der Unterschiedsschwellen für den Geschmackssinn bei Reizzunahme und Reizabnahme, *Arch. f. d. ges. Physiol.*, 197, 1922, 337.

[4] Although Fodor and Happisch used a rather extended stimulus-range, they made no attempt to obtain exact liminal values.

[5] W. Brown, The judgment of very weak sensory stimuli, *Univ. Calif. Publ.*, 1, 1914, (no. 3), 202.

[6] Brown, *op. cit.*, 204-205, and references there cited. The relationship between saline reaction-time and (1) concentration, (2) temperature, (3) pressure, and (4) amount of taste-cell substance is now being investigated by the authors.

[7] The salt used in this experiment was that prepared by the J. T. Baker Chemical Co., Phillipsburg, N.J., under the label Sodium Chloride. C.P., M.W. 58.45.

[8] Von Skramlik, *op. cit.*, 401, 487, 493.

inorganic salts produce complicated tastes in varying degrees.[9]) With a definite upper limit of solubility at 36 gm. per 100 cc. of water,[10] sodium chloride is easily graded quantitatively and can be varied over a relatively wide range between two finite concentrations (from zero to 5.42 mol. concentration: see footnote 46). Experimentation with *sour,* on the other hand, is restricted to a relatively narrow range inasmuch as stimuli for the sour process (acids) produce an "astringent sensation" at concentrations greater than 0.1 mol.[11] In experimentation with *sweet,* various difficulties are encountered in the choice of a suitable stimulus,[12] as may be seen in the highly variable results Lemberger obtained in her investigation of this process.[13] Unlike the stimuli to *bitter* (*e.g.* quinine), which cling and adhere to the tongue and produce after-effects of long duration,[14] the taste of salt can be made to disappear relatively quickly.[15] Moreover, salt solutions have no disagreeable effects and, even after repeated tasting, produce none of the strong feelings which attend bitter, sweet, and sour stimulation.[16]

Although, as Hänig and others have shown,[17] all the taste qualities are perceived on all parts of the tongue provided the concentration of the solution is sufficiently high, nevertheless, the peripheral field shows variations in sensitivity with respect to the four fundamental qualities. The tip of the tongue, the area used in this experiment and the region most readily accessible for stimulation, has been shown by Hänig and Hamelinck to be highly sensitive to salt as well as to sweet.[18] Furthermore, the threshold values obtained by Kiesow for NaCl on different regions of the tongue indicate that the borders of the tongue are not necessarily more sensitive to salt than is the tip of the tongue.[19]

Inasmuch as gustatory sensitivity is best investigated with a substance that does not change the qualitative response when different intensities of the stimulus are presented, qualitative modulation must be considered. Renqvist reported that a qualitative change seems to occur definitely between the concntrations 0.009 mol. and 0.02 mol. (and possibly as high as 0.03 mol.) with common salt, which tastes sweet and not salt between these two limits.[20] Since the *Os* in the experiment here reported were 'set,' however, for the process *salt (vide infra)* and since the threshold values obtained were 0.025 mol. and 0.05 mol. respectively for the two *Os*, the

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[9] H. Oehrwall, Untersuchungen über den Geschmackssinn, *Skand. Arch. f. Physiol.* 2, 1890, 1. Cited by Von Skramlik, *op. cit.*, 449.

[10] J. W. Mellor, *Modern Inorganic Chemistry,* 1927, 270.

[11] A. Herlitzka, Sul sapore metallico, sulla sensazione astringente e sul sapore dei sali, *Arch. di. Fisiol.*, 5, 1908, 217. Cf. Von Skramlik, *op. cit.*, 426.

[12] Brown, *op. cit.*, 204, footnote.

[13] F. Lemberger, Psychophysische Untersuchungen über den Geschmack von Zucker und Saccharin (Saccharose und Krystallose), *Arch. f. d. ges. Psysiol.*, 123, 1908, 293.

[14] E. B. Titchener, *Experimental Psychology,* vol. 1, part 2, 1901, 101; Von Skramlik, *op. cit.*, 469, 489.

[15] Von Skramlik, *op. cit.*, 489. [16] Brown, *op. cit.*, 204.

[17] D. P. Hänig, Zur Psychophysik des Geschmackssinnes, *Philos. Stud.*, 17, 1902, 576. Cf. Von Skramlik, *op. cit.*, 478, for list of other *Es*.

[18] D. P. Hänig, *op. cit.*; M. Haemelinck, Sur l'assymetrie gustative, *Année psychol.*, 11, 1905, 117.

[19] F. Kiesow, Beiträge zur Psychologie der Geschmackssinnes, *Philos. Stud.*, 10, 1894, 329.

[20] Y. Renqvist, Der Geschmack, *Skand. Arch. f. Physiol.*, 38, 1919, 97. Cited by Von Skramlik, *op. cit.*, 401.

occurrence of qualitative modulation (Geschmackswechsel) presents no problem here.

Precautions. In order to decrease the variability of the results, the following precautions were observed.

(1) The mouth was thoroughly cleansed after eating, and a 2-hr. interval was allowed to intervene between the last meal and the beginning of an experimental session. The *O* was permitted only water during this interval.[21] To avoid 'overexertion' the recommended 1-hr. experimental session in a half-day was approximately observed.[22] The length of the experimental session varied, however, since the *O*s needed 'rest' periods more frequently at high stimulus-concentrations than at low concentrations. Five-minute relaxation periods were introduced after 25 reports. Rest periods were also permitted whenever *O* reported increased uncertainty in judging, whenever *O* asked outright for a rest, and whenever *E* observed by inspection that the recorded judgments were becoming inconsistent with the previously established criteria (*vide infra*).

(2) To prevent stimulation of the olfactory organ the *O*s' nostrils were stopped with cotton as recommended by Chevreul.[23]

(3) Inasmuch as tobacco smoking is believed to decrease sensitivity,[24] both *O*s, who are smokers, abstained on days when experimentation was in progress.

(4) Each *O* was asked to dry his tongue by squeezing it against the roof of his mouth before extending it.[25] Since taste solutions may be diluted by saliva,[26] this precaution served to remove the excessive saliva from the area of stimulation and reduced it to a minimum.

(5) All time factors were kept relatively constant. For, although the flow of saliva is independent of (a) the quantity of the taste substance and (b) its concentration, it seems to be related to the duration of the stimulus-presentation and to the time-interval between successive presentations.[27] The duration of a single stimulus-presentation was 10 sec. Preliminary experiments (approximately 25 hr. for each *O*) indicated that this presentation is optimal for stimulation at moderate concentrations when a single drop of solution is used.[28] The rinse after the stimulation lasted 10 sec. (on the average) and the interval between successive stimulations was about 75 sec.[29]

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[21] Camerer, Die Grenzen der Schmeckbarkeit von Chlornatrium in wässriger Lösung, *Arch. f. d. ges. Physiol.*, 2, 1869, 322; Titchener, *op. cit.*, 99.

[22] Von Skramlik, *op. cit.*, 489.

[23] G. Chevreul, Des differentes manieres dont les corps agissent sur l'organe du gout, *J. de physiol. exper.* 4, 1824, 127.

[24] S. Ottolenghi, Il gusto nei criminali in rapporto coi normali, *Arch. di psichiat.*, 10, 1889, 332. Cited by Von Skramlik, *op. cit.*, 500.

[25] Titchener, *op. cit.*, 101. "With care the squeeze against the roof of the mouth answers well"—this with reference to ridding the tongue of saliva. Titchener pointed out that although the tongue may be dried by pressing with cotton wool or with a fine cloth immediately upon exposure, this procedure resulted in a too speedy evaporation and a consequent reduction in the sensitivity of the papillae.

[26] Von Skramlik, *op. cit.*, 386.

[27] E. Gertz, Untersuchung über die Reizschwellen des Coffiens und Theobromins, *Skand. Arch. f. Physiol.*, 44, 1923, 129. Cf. Von Skramlik, *op. cit.*, 387 ff.

[28] Von Skramlik recommends that the stimulus application be 20-30 sec., *op. cit.*, 489.

[29] Von Skramlik states that an interval of 1-3 min. between stimulus presentations

(6) In the experiments here reported, the solutions were maintained at room-temperature, 18° C.[30] (The extreme variations were less than 3° C.) There is considerable disagreement as to the temperature of stimulus-solution to be used in taste experiments.[31] The temperature-range most favorable for taste perception is variously given as 10-20° C. (for salt),[32] 20-35° C.,[33] 10-35° C.,[34] 30-40° C.,[35] 20-37° C.[36] Other experimenters have stated that threshold values remain uninfluenced within the limits of 0° to 50° C.,[37] and that the gustatory sense has no temperature coefficient.[38] More recently it has been shown that salt is best tasted at 18° C. (at concentrations above threshold values),[39] and that, at the threshold, salt stimulation becomes two to three times as effective when the temperature of the solution decreases from 42° to 17° C.[40] Our experimentation was thus carried out within the limits of a temperature-range where sensitivity is greatest (if we accept the results of the two more recent investigations), while maintaining a constant relationship between the temperature of the solution and the 'physiological zero' of the experimental surface.[41]

(7) It is essential that the tongue remain passive throughout the period of stimulation, if a constant area is to be stimulated: it has been shown that, with the application of solutions of equal concentration, the strength of the sensation varies directly with the size of the stimulated surface.[42]

(8) Following Brown's suggestion, we sought to standardize the rinse by irrigating the experimental area for a period of about 10 sec. Although the amount of rinse water is a minor factor,[43] it may assume importance when an effort is being made to keep conditions perfectly uniform. Rigid standardization, however, is not possible. At low concentrations 10 sec. is unquestionably too long a time, and at high concentrations (4 mol.) it may not be long enough. At high concentrations, therefore, a rinse of longer duration was frequently permitted.

Apparatus. The apparatus consisted of 5 glass burettes (50 cc. capacity) into

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ought to suffice if after-effects are to be avoided. We selected as satisfactory the shortest of these times (approximately), on the basis of our earlier preliminary work.

[30] Brown, *op. cit.*, 207.

[31] It is an established experimental fact that extreme temperatures produce complete desensitization of the taste sense, disturbing at the same time the entire sensory mechanism of the tongue, *i.e.* all other sensory modalities are affected. F. Kiesow, Beiträge zur Physiologie des Geschmackssinnes, *Philos. Stud.*, 12, 1896, 464.

[32] Camerer, *loc. cit.*

[33] Béclard, *Traité elementaire de physiologie humaine*, 1866. Cf. Von Skramlik, *op. cit.*, 501.

[34] H. Beaunis, *Recherches expérimentales sur les conditions de l'activite cerebrale et sur la physiologie des nerfs*, 1885, as cited by Von Skramlik, *loc. cit.*

[35] Schreiber, *loc. cit.*; H. Henning, Physiologie und Psychologie des Geschmacks, *Ergeb. d. Physiol.*, 19, 1921, 50.

[36] Von Skramlik, *op. cit.*, 489.

[37] F. Kiesow, *loc. cit.*; L. Chinaglia, Richerche intorno all' influence esercitato della temperatura sulla sensibilita gustativa, *Riv. di psich.*, 11, 1916, 196.

[38] K. Komuro, Le sens du gout a-t-il un coefficient de temperature? *Arch. néerl. de physiol.*, 5, 1921, 572. Cf. Von Skramlik, *op. cit.*, 501.

[39] Gondriaan, *Arch. néerl de physiol.*, 15, 1930, 253.

[40] H. Hahn and H. Günther, Über die Reize und die Reizbedingungen des Geschmackssinnes, *Archiv. f. d. ges. Physiol.*, 231, 1932, 48.

[41] Brown, *op. cit.*, 207.

[42] W. Camerer, Über die Abhängikeit des Geschmacksinnes von der gereizten Stelle der Mundhöhle, *Zsch. f. Biol.*, 6, 1870, 440. Cf. Von Skramlik, *op. cit.*, 482.

[43] Brown, *loc. cit.*

which were introduced the 5 solutions used as variable stimuli. Each of the burettes, appropriately labelled, was mounted in a upright position on a horizontal rotating standard (72° apart). A rod clamped to the standard enabled *E* to rotate it and to present the stimuli in any desired order. *E* sat opposite *O*. The apparatus was on a table between them.

The burettes were tilted at an angle of approximately 70° to the horizontal, so that they could be readily brought over the region selected for experimentation, an area in the center (laterally) of the tongue and 1 cm. from its tip. The height of the burettes was adjusted for each *O* so that the separation between the tip of the burette and the experimental area on the tongue was about 0.5 cm. The required amount of stimulus solution (one drop) was released by opening and closing the glass petcock. A felt lined chin-rest was used to maintain the *O*'s head and tongue in a fixed position during stimulation. A metal trough leading to a basin stood directly beneath *O*'s chin-rest, and was used to drain off all the expelled saliva and rinsing solution. This set-up afforded an efficient and convenient means for stimulating an area sufficiently large to provide a reasonably wide intensitive range.

Method. The psychophysical method was the method of *single stimuli*.[44] The validity and usefulness of the method have been demonstrated several times.[45] This method, which calls for a judgment following the presentation of one stimulus only, is particularly appropriate for psychophysical work in taste. Many difficulties inherent in the methods heretofore available for gustatory studies are avoided and there is a consequent increase in accuracy. It is difficult to make simultaneous comparisons in taste, and neighboring regions of the tongue are apt to vary in sensitivity. Other errors attendant upon immediately successive presentations of two stimuli (cf. the method of constant stimuli) are also avoided.

The *O* was instructed to respond with reference to the intensity of the salt process, *i.e.* to report whether the taste afforded by the stimulus was weak or strong relative to a constant and critical strength for a given series of presentations. This criterion ('standard' or 'absolute' series) was readily provided in each series by short practice periods. Previously to the experiment reported here, however, both *O*s had devoted more than 25 hours each to preliminary experimentation. The *O*s were thus disposed strictly to report upon the intensity of a process called 'salt' (1) by instruction and (2) by training.

Psychophysical procedure. We used a basic NaCl solution of 5 mol., diluting it at the time of experimentation to the various concentrations required.[46]

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[44] J. Volkmann, The Method of single stimuli, this JOURNAL, 44, 1932, 808 f.

[45] C. Pfaffmann, An experimental comparison of the absolute and constant stimulus methods in gustation, *Psychol. Bull.*, 31, 1934, 622; E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493; S. W. Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578; C. C. Pratt, Time-errors in the method of single stimuli, *J. Exper. Psychol.*, 16, 1933, 798-814; M. J. Zigler and A. H. Holway, Differential sensitivity as determined by amount of olfactory substance, *J. Gen. Psychol.*, 12, 1935, 372-382.

[46] A molecular solution of NaCl contains 58.45 gm. of NaCl per liter of *solution*. The solvent is water. In this connection it may be noted that Von Skramlik in converting concentration figures from percentages into molar concentrations proceeds incorrectly. A statement of 'percentage-concentration' signifies the amount of substance dissolved per unit volume of solvent, so that the volume does not remain constant at one liter: *e.g.* 10 gm. NaCl per 1000 cc. of water is a 1% solution, 20

Sensitivity was tested at the absolute threshold and at 0.5, 1.0, 2.0, 3.0, and 4.0 mol. concentrations. Five equally separated stimulus-amounts were used as variables in determining a psychometric function at each of these points. The size of the step between variables for a given series was selected empirically on the basis of its promise to yield a satisfactory psychometric function. The criteria for such functions were laid down as: (1) *no inversions* and (2) the largest p (relative frequency) to be less than 100% but not less than 75%. These criteria entailed much preliminary experimentation and trial-and-error procedure. Occasionally, an entire experiment for a single series was carried through to completion and found wanting in respect of them. In seven instances, this failure meant changing the size of the step and repeating the experiment. The separations which met fully the above criteria are:

	Mol.					
Stimulus concentration	0.0250	0.05	1.00	2.00	3.0	4.0
Separation between stimuli	0.0125	0.15	0.25	0.25	0.4	0.5

At the 'ready' signal the *O*, blindfolded, moved forward from a relaxed position, and adjusted his chin comfortably to the rest provided for it. He extended his tongue about 2.5 cm., after 'drying' it by pressing it against the roof of his mouth. *E* then signalled with the word "ready" that he was about to present the taste-substance. The tongue was stimulated with a *single drop* of NaCl solution by careful manipulation of the petcock.[47] After a 10 sec. presentation (stop-watch), *E* called for the report which *O* gave by tapping the table with a pencil if the intensity of the *salt* process was 'strong.' The failure of *O* to tap signified that the intensity of the *salt* process was 'weak.' This form of response was preferred to verbal report because it seemed desirable that the tongue should be rinsed before it was retracted, and that the response should be based only on the 10-sec. presentation. No intermediate category was used. *E* used a rubber-bulb syringe containing distilled water to rinse the salt solution from *O*'s tongue, and then recorded the judgment. After the rinse, the *O* spit the excess fluid into the trough, and returned to a relaxed position for a period of 1 min., while *E* rotated the standard to bring the next stimulus-variable into position. Twenty reports were secured for each of the 5 variables. Each psychometric function was calculated on the basis of 100 presentations. A random order was used in presenting the variables throughout a given series.

Results

Qualitative. Pressure was always reported when the *O* was set to make a report relative to apparent qualities. It is quite unlikely, however, that the differences in weight existing among the various drops produced anything more than a slight but unappreciable constant error in the final results. *O*s sometimes reported the presence of warmth or cold at the beginning of an experimental series; however, these reports seemed to be about equally dis-

gm. per 1000 cc. is a 2% solution, etc. Since the total volume varies, it is incorrect to divide the salt quantity by the molecular weight to obtain the true molecular concentration of the solution.

[47] The volume of the average of 250 drops was 8.20 cu.mm.

tributed for the 5 variables of a given series, and are accordingly constant in effect upon the psychometric functions as a whole. Smells were effectively precluded by stopping the nostrils with cotton (*vide supra*).

Pain did not occur for low concentrations. At 3 mol., the *O*s reported the presence of 'sting' in 12% of cases (N = 200).[48] At 4 mol., 'sting' appeared in 60% of the reports. 'Sting' seems to be the rule for a 10-sec. application of a drop of NaCl solution when concentration equals 4 mol. Typically 'sting' resembles 'prick' pain. A representative description follows. "The impression has extent. It is small. Its boundaries are poorly defined, though they are much more definite than are those for salt alone. In the center there are points which are 'sharp,' 'biting,' 'nippy,' 'stingy.' "

The total impression forms a distinct qualitative pattern. There is (1) a small saline area which encloses (2) a congeries of sting points, and which is encompassed by (3) a ring of light pressure.

Intensitive. A sensory process varies in time.[49] The process *salt* has a temporal course. Under special conditions, it varies in time most clearly in respect of intensity. Consistently to regard *salt* as a *process,* we must control the time-factor (which usually receives but little attention in psychophysical experiments), and we must also study the intensive variation of the process as a function of time.

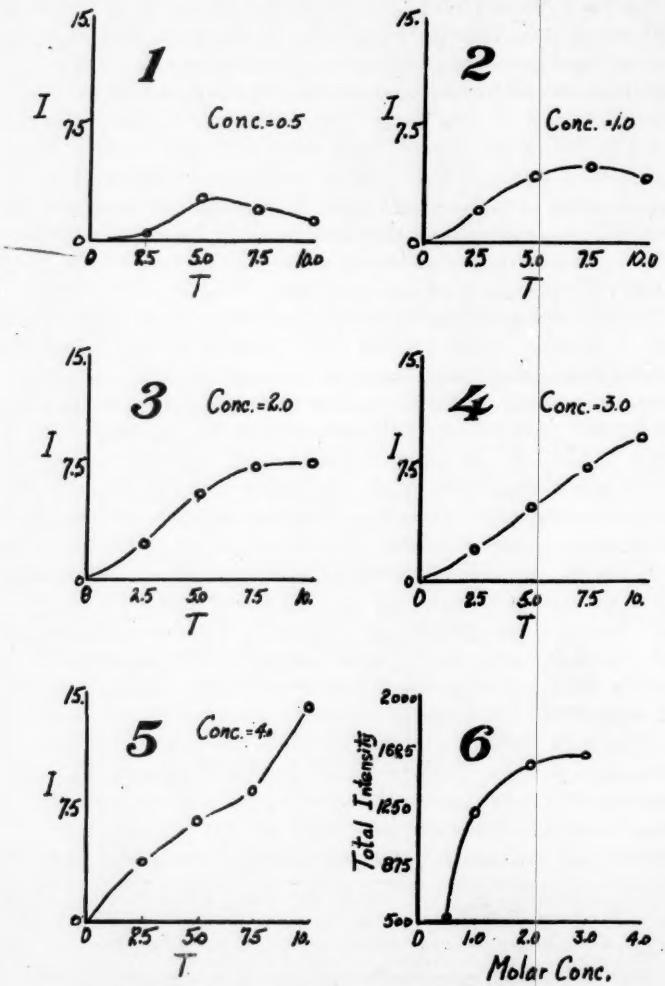
To this end, we undertook to obtain records of the way that the intensity of the *salt*-process varies as a function of time during a period of 10 sec.[50] The technique employed was quite simple. The *O* was given a pencil and a sheet of graph paper. The abscissa represented the time-axis; the ordinate, the degree of saltiness. The *O* was instructed to place the point of the pencil on the intersection of the two axes, and to trace the intensity of the process on the graph paper. At the end of the 10-sec. presentation, he was stopped abruptly. With practice, the *O*s attained a degree of adeptness which may be accepted *pro tempore* as satisfactory both as regards (1) confining the records within reasonable limits and (2) producing very rough but fairly consistent results.[51] The practice-series were conducted with con-

[48] There is nothing novel in the production of pain by the use of concentrated salt solutions. Cf. Von Skramlik, *op. cit.,* 425, 430, 438, and references there cited.

[49] Cf. E. G. Boring, *The Physical Dimensions of Consciousness,* 1933, 144.

[50] These experiments on intensitive variation as a function of time were performed *after* the psychophysical experiments which employed the method of single stimuli. For the sake of clarity, their order is reversed in exposition.

[51] We have no results that bear upon the latency of the salt process. Latency could be measured, however, if the pencil were placed in a straight, narrow, vertical groove, and the graph paper were moved horizontally past the groove at a constant speed by means of a revolving drum. In this way more reliable results would be obtained since the *O* would have to record only the vertical component of the intensitive-temporal complex.



FIGS. 1-6. INTENSITY AS A FUNCTION OF TIME

The first 5 plots show intensity (I) expressed as a function of time (T) in sec. Each circle is the average of 20 observations. Fig. 6 depicts the *total* intensity of the sensory process plotted as a function of molar concentration: these points are the integrals of Figs. 1-4; see text.

centrations of 0.5 mol. and 4.0 mol. in order to adjust the *Os* to the intensitive limits. In the experiment proper, 10 samples from each *O* were obtained for the mean value of each of 5 series which were used in the phychophysical experiment.[52]

The abscissae were divided into four equal parts, and the intensitive records were replotted on the same baseline at intervals of 2.5, 5.0, 7.5, and 10 sec. The 10 intensity-values (measured in cm.) corresponding to these four abscissae values were then averaged for each *O*, and the averaged values for a particular concentration of salt were then averaged for both *Os*, plotted, and 'smoothed.' The resultant graphs illustrate in a general way the manner in which intensity varies as a function of time for a period of 10 sec., and for a stated concentration. For these intensitive diagrams see Figs. 1–5.

The first diagram (Fig. 1) shows the typical course of intensity for a period of 10 sec., as occasioned by a drop of solution of concentration 0.5 mol.[53] Fig. 2, indicates that maximum intensity is reached later, for a concentration equal to 1.0 mol., than for a concentration equal to 0.5 mol.—although the initial slope of the former is apparently greater than that for the latter. Intensity begins to decline about 7 sec. after a concentration of 1.0 mol. has been presented. Fig. 3 shows no decrease in intensity for 2 mol. though the small difference between the intensity at 10 and intensity at 7.5 sec. may mean that it is nearing its maximum. The slightly sigmoid form of the points plotted in Fig. 4 (3 mol.) makes it consistent as an intermediate case between Fig. 3 and Fig 5. An alteration occurs in the rate of change of intensity in Fig. 5 (4 mol.) where the final increase may be correlated definitely with the appearance of the report 'sting.'

These five diagrams suggest the possibility that the intensive criteria may change even when an *O* is adequately disposed to discriminate strictly with regard to intensity, and our protocols confirm this view. The *Os* found that they were reporting upon the process *as a whole* for high concentrations (greater than 0.5 mol.), whereas at low concentrations they judged in terms of *maximal* saline intensity.[54] This fact implies that the area under

[52] An 'attempt' was made, however, to estimate the duration of the latent period. *E* started a stop-watch at the moment the stimulus-drop struck the tongue and then tried to stop it at the moment when *O* began to move the pencil. The attempt failed for, above 0.5 mol., *O* began to move the pencil almost as soon as the drop of solution touched his tongue.

[53] The latent periods which must exist for this and all other intensitive diagrams have been neglected.

[54] In the psychophysical experiment the reports issued automatically: the nature of the experiment prohibited deliberate judgments. It was only by means of this special instruction that these intensitive diagrams were made possible.

the curve in a particular diagram (the 'total intensity' involved in a 10 sec. presentation) probably serves critically to determine the psychophysical report, when the tongue is impressed with a concentration greater than 0.5 mol. for a period of 10 sec.

Another use of the intensitive diagrams is the light which they cast on the problem of the nature of the intensitive stimulus for salt. Ordinarily the concentration of the gustatory substance is regarded as the stimulus to intensity. A glance at any one of the intensitive diagrams shows, however, that, while the concentration originally placed on the tongue is 'fixed,' the

TABLE I

PSYCHOMETRIC FUNCTIONS OF GUSTATORY INTENSITIVE DIFFERENCES FOR SALINE SOLUTIONS

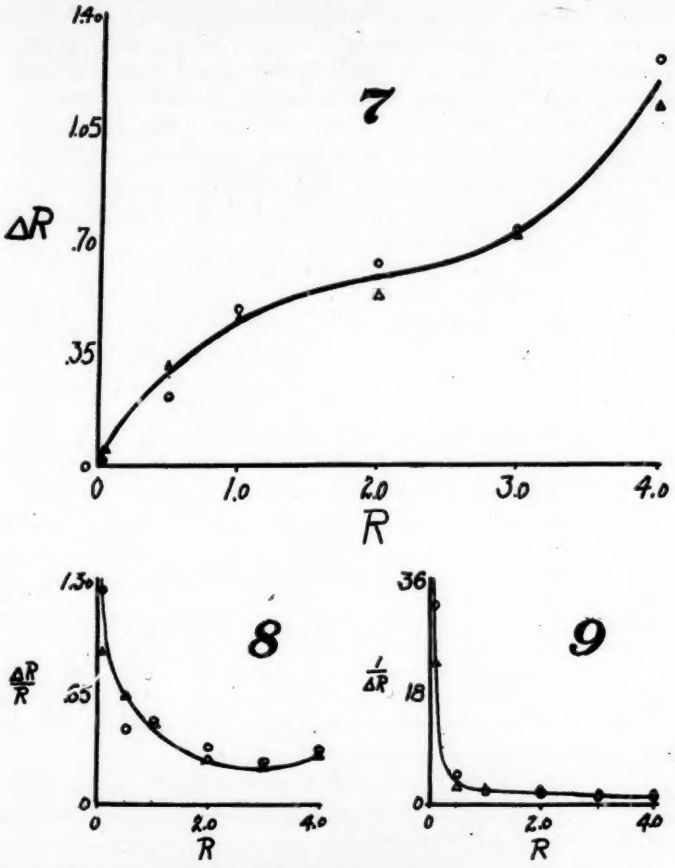
Relative frequencies are based on 100 replies for each stimulus-series.

R Mean value of stimulus-series (mol.)	O	Range of stimulus-values	Relative frequency of 'strong' replies for the different stimuli					h	ΔR	ΔR/R
			1	2	3	4	5			
.025	A.H.H.	0–.05	.25	.35	.40	.55	.90	22.62	.03	1.25
.050	L.M.H.	0–.10	.15	.30	.60	.80	.85	15.75	.04	.90
.50	A.H.H.	.2– .8	.10	.25	.60	.80	.90	3.17	.22	.45
	L.M.H.		.30	.45	.55	.85	.90	2.23	.32	.63
1.00	A.H.H.	.5–1.5	.15	.30	.45	.70	.85	1.45	.49	.49
	L.M.H.		.15	.25	.50	.70	.85	1.52	.46	.46
2.00	A.H.H.	1.5–2.5	.15	.35	.55	.60	.75	1.12	.63	.32
	L.M.H.		.25	.35	.55	.70	.95	1.33	.53	.27
3.00	A.H.H.	2.2–3.8	.05	.30	.50	.55	.85	.96	.73	.24
	L.M.H.		.25	.40	.60	.80	.95	.97	.73	.24
4.00	A.H.H.	3.0–5.0	.25	.35	.45	.60	.85	.56	1.27	.32
	L.M.H.		.25	.35	.60	.75	.85	.64	1.11	.28

intensity varies in a definite manner from moment to moment. Saline intensity depends upon time as well as concentration.

On the other hand, special considerations indicate that the traditional designation is not wholly without justification. Fig. 6, for example, shows that the total intensity for the *salt process* varies directly as a function of the concentration originally placed on the tongue. The relationship is not linear, nor is it exactly logarithmic. A semi-log plot suggests that the function may be ogival in form.

Psychophysical. Table I contains all the psychophysical values upon which further statements concerning the mechanism of the salt-process are to be based. The significant relations existing among the values entered in



FIGS. 7-9. DIFFERENTIAL SENSITIVITY

All curves are smoothed averages for two *O*s. Fig. 7 shows ΔR (a variation-index) as a function of R (in units of molar concentration). Fig. 8 illustrates the course of the Weber-ratio as a function of R: the path is completely 'curvilinear.' The minimal value of ΔR/R is correlated with the introduction of a complicating qualitative factor ('sting'). See text. Fig. 9 illustrates the hyperbolic form of the function relating differential sensitivity and stimulating agency.

this table, for both *O*s, may be perceived with greatest economy, visually: these relations are illustrated in Figs. 7-9.

Fig. 7 shows the general manner in which ΔR varies as a function of the

stimulus, expressed in terms of molar concentration (R). The slope of the curve decreases continuously up to a concentration which is in the neighborhood of 3 mol., after which the direction of the rate of change in the curve appears to alter. Something of this sort was not entirely unexpected, however, since both *O*s had (previous to the computation of ΔR) reported the occurrence of 'sting' in 60% of the cases, and since both *O*s had also previously indicated that at a concentration of 4 mol. they were no longer judging only in terms of saline intensity alone but in terms of the intensity of a 'salt-string' complex.

Fig. 8 illustrates the manner in which the Weber-fraction varies as a function of the strength of the taste substance. The curve decreases rapidly at first, reaches a minimum, and tends slightly to increase at the highest concentration. The minimal ratio is, of course, approximately coincident with the region at which 'sting' tends to appear, because the minimum in Fig. 8 would coincide with the point of inflexion in Fig. 7.

The value, $1/\Delta R$, represents the 'differential sensitivity' of the mechanism involved in salt reception. Fig. 9 shows the way that $1/\Delta R$ varies with the stimulus concentration. The function is definitely hyperbolic in form.

A distinguishing feature of these psychophysical results lies in the fact that the curves for the two *O*s, paralleling each other very closely, signify that a definitely reproducible function of the human organism has been described.

Summary

(1) The qualitative spatial-temporal pattern of the gustatory process arising from stimulation by sodium chloride of different concentrations is described. It is this pattern which must include the basis for the judgments from which the psychophysical constants are derived.

(2) A new technique for descriptive report is described. It provides a picture of the temporal course of intensity during the period in which the psychophysical judgment is formed.

(3) The method of single stimuli has been employed to determine the differential intensitive sensitivity of two *O*s for salt at five intensities, that is to say, throughout the complete range through which the saline taste remains uncomplicated by the addition of 'sting.'

(4) The course of the Weber-fraction for salt as a function of salt-concentration is described. The region of minimal relative inertia is correlated with the introduction of another modality (pain).

SOCIAL SIGNIFICANCE

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Notwithstanding the flourishing condition of social psychology today, it is noteworthy that one finds competent judges expressing disappointment over the fact that it has made little or no progress during the past quarter-century. In this connection the remarks of F. C. Bartlett come to mind. "In its modern aspects," says Professor Bartlett, "social psychology had a brilliant beginning when William McDougall wrote his *Introduction to Social Psychology,* but since then it seems to have made very little advance."[1] For this static condition he finds McDougall's book, despite its originality and value, largely to blame, since by its emphasis upon instinct in social behavior it led to a long period of fruitless controversy in the field of social psychology between instinct psychologies and psychologies of every other kind. As surprising and unwarranted as such adverse judgment is bound to seem to many workers and writers in this field, there is, as I believe, fairly good ground for it. In current treatises and textbooks on social psychology, which commonly run to a great length, one finds an amazing variety of topics discussed, from the processes involved in organic heredity and the structure and organization of the human nervous system to the higher forms of human culture and the principles of social psychiatry. But what one misses is a penetrating and illuminative analysis of the psychological factors involved in human association, a real 'explanation,' taken in this sense, of the psychology of social living.

A noteworthy exception is to be found in certain writings of Madison Bentley, which seem to supply the basis for such an analysis.[2] This writer starts with the understanding that the 'social' qualification of living inheres in objects, events, observed human relations, and the like, and not in the

* Accepted for publication June 30, 1935.

[1] F. C. Bartlett, *Remembering,* 1932, 239.

[2] Compare M. Bentley, A preface to social psychology, *Psychol. Monog.,* 21, 1916 (no. 92), 1-25; *The Field of Psychology,* 1924, 455-490. An elaboration of the point of view here considered has been derived from the same author's *The New Field of Psychology,* 1934, 1-31; *The Problem of Mental Disorder,* 1934, 275-308, and from personal exchanges with the present author. Further indications of a more fruitful way of regarding social facts in psychology are to be found in M. F. Washburn, Ejective consciousness as a fundamental factor in social psychology, *Psychol. Rev.,* 39, 1932, 395 ff., and in The social psychology of man and the lower animals, *Studies in Psychology: Titchener Commemorative Volume,* 1917, 11-17, as well as in R. Dodge, Mental nearness, *J. Abn. & Soc. Psychol.,* 28, 1933, 233-244.

mind or consciousness of an individual or a group or in hereditary instinctive or dispositional possessions of the socialized organism. Thus 'social' is seen to be *attributive to the products or issues* of perceiving, imagining, desiring, emoving, comprehending, and the other psychological activities. In this way, the anticipation of a friendly greeting, as I open a letter in my private mail, is on the same level of descriptive fact as the comment that the ring accords well with evening wear. To go behind these plain facts and to fabricate a 'social consciousness' is, for Bentley, both unnecessary and mischievous; and to ignore the real social qualification of objects and occurrences by the phrase 'social stimulus,' borrowed from an alien context, is to evade the central problems of social psychology. The organism has to *make* its sociality without a magical 'social mind'; and it cannot 'respond' to it before it is made. As for the context of social facts, Bentley finds this in two forms, in *the congregate, i.e.* any plurality of actively associated individuals, and in *the consociate, i.e.* the 'belonging' reference which is constantly enforced among adults and children by custom, lore, rule and law, and by man's cultural and institutional setting. Congregates are typical social 'objects.' Their describable varieties include the pair, the throng, the audience, the military formation, the play-group, and many others. They are social integrations of a psychological kind. In the consociate, the primary quality appears to the organism as a belonging, an inhering; and this quality, again, is intrinsically social. It is no more a magical instinct, drive, or racial guide which underlies conformity to, or rebellion against, the tribe or state than is plain membership in one of the congregates.

This view, which tends to make social psychology factual and descriptive, tries to avoid both the problems of sociology and the confusing theories of general biology and philosophy. Having once found and delineated the social aspects and qualifications of the organism's products ('objects' taken in the widest sense), it accordingly seeks out the conditions—within the body, among outside occurrences and influences, and in human history—which underlie and govern these social productions. Here social psychology proceeds just as psychologists at large proceed when they study thinking, remembering, imagining, and other activities of the living organism. While its method and its principles are those of general psychology, the emphasis of social psychology is on the congregated and the consociated aspects of living and of the many factors which underlie these aspects.

Now since the life of man is so constantly and inveterately congregate and consociate, we must allow for 'social tuning,' that is to say for the

constant and multiform social qualifications of our daily living. Having forsaken social instincts and the like, Bentley naturally turns for a support of this tuning to bodily trends, habituational promptings, determining tendencies of the neurologist, and the like. While generally commending the empirical point of view here sketched, the present writer would like to deal specifically, though along a somewhat different line, with these supporting tendencies of socialized behavior in order to emphasize their functional importance in the creation of social significances. Three types of tendency appear to him to be principally instrumental in giving this kind of significance to perceived or imagined objects.

The first of these tendencies consists of our speech-habits. When one responds to the pattern of stimulation set up by objects or situations by naming or describing the objects or situations to himself (as is usually the case with inspective perception) that response imparts social significance to the objects which evoked it. In proof of this, it should be sufficient to refer to the fact that articulate speech as a social product necessarily reflects interests and activities common to a group which in the course of its social history developed a language. But more explicit statement is no doubt desirable. Putting the matter very clearly from the behaviorist's standpoint, Mead[3] pointed out that gestures, including, of course, language-responses, arose in organic behavior as early indications (*i.e.* symbols) of actions about to be performed or that may be performed. In thus indicating to himself the reaction the object may call forth, the individual indicates to himself its meaning. But it is not enough, Mead insists with emphasis, that the individual should by customary gesture indicate this meaning as it exists for himself alone, but that he should indicate this meaning as it exists for others to whom he is pointing it out.

> "We must indicate to ourselves not only the object but also the readiness to respond in certain ways to the object, and this indication must be made in the attitude or rôle of the other individual to whom it is pointed out or to whom it may be pointed out. If this is not the case it has not that common property which is involved in all significance. It is through the ability to be the other at the same time that he is himself that the symbol becomes significant."[4]

Mead would therefore hold that in a very real sense the plurality of observers is always implied in the use of the significant symbol. So far as the social significance of objectively-meaningful speech is concerned, the essential fact can, I think, be stated more clearly and effectively in other

[3] G. H. Mead, A behavioristic account of the significant symbol, *J. Philos.*, 19, 1922, 157 ff.
[4] Mead, *op. cit.*, 161.

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terms. When objects and situations touch off our verbalizing tendencies or speech-habits, what we respond to in these situations are features (quality-patterns and event-sequences including those of human action) which continually recur in our own experience and the experiences of others. These speech-responses have social significance because they signify features common to the experience of myself and my human associates. While it is by no means established that speech-responses are necessary for the singling out of the recurring features and configurations of the external world, it is certain that they do make perceived objects serviceable to human needs and that, so far as operative, they impart social significance to objects and events. The word *stove* still means to me the 'Round Oak' wood-burner I saw daily as a child in the home living-room and in other houses of the district where I grew up. Whether the symmetrical shape, familiar from infancy, or the word spoken in warning from so early a time, came first it would be hard to say, but there is no doubt that the word, when applied to the object, still imparts to it some remnants of its early social meaning when it signified a dependable source of warmth and comfort to all members of the family and to friends and neighbors as well.

A second type of determining tendency which, when called into play by an object or event, gives to it a social significance, is to be found in those habitual interests, acquired industrial skills, and political, religious, and family sentiments produced in the individual by the dominating influence of social custom and tradition. We know that the behavior of the human individual in the various situations of his daily life is largely determined by the customs and interests of his social group. But it has not been sufficiently realized, as I think, how largely social customs and standards determine the individual's perceptions of the objects and situations he encounters, and what these observed objects bring up through the processes of recall. Bartlett has recently supplied convincing proof of the importance of these acquired ways of acting, feeling, and thinking in determining the meaning which objects and events have in perception and recall.[5] The evidence he presents was obtained both from experimental studies in his laboratory and from carefully-checked observations of the behavior of a native African tribe. His principal concern was with the memory-response, but his conclusions bear almost as directly upon the processes of perception and recognition which are inseparably linked with those of recall. Undertaking to test the numerous stories of the marvellous word-perfect memory of the Swazi, a group of Bantu natives living in the Transvaal, he found that when individuals were given the kind of test for recall

[5] F. C. Bartlett, *op. cit.*, 247-255.

usually employed in the laboratory, they did neither better nor worse than the average European. But when the matter for recall was taken from the natives' preferred field of interest the results were very different. Most Swazi culture revolves around the possession and care of cattle, hence cattle are the centre of their most persistent and important customs. When Bartlett tested the memory of the Swazi in this field, he found the individual herdsman generally to have an accurate and prodigiously rententive capacity to recall the individual characteristics of his beasts. In discussing the import of such facts as these, Bartlett emphasizes a point which bears directly upon the subject we are here considering. He refers to the fact generally acknowledged that all human reactions have a social frame or background into which they must fit. But, he points out, such a background is never effective in determining action merely by 'being there,' *i.e.* by its mere presence, repetition, or persistence. Rather is every social group organized and held together by tendencies which construct the special, persisting features of its group-culture—its technical and religious practices, its material art, its traditions and institutions. This, the specific bias in the group, expressing itself in the system of social culture, awakens in the individual a tendency to notice, retain, and construct along certain lines. Thus the social influence does "immediately settle what the individual will observe in his environment, and what he will connect from his past life with this direct response."[6]

A third kind of tendency which imparts social significance to perceived or imagined objects is that which determines the mode of emotional expression. It is now agreed that our expressive emotional responses to objects and situations do not follow any strict, hereditary patterns but are largely habits acquired in the course of individual experience. These acquired tendencies reflect the influence of social example and of current modes of literary portrayal and of dramatic representation. They possess, to be sure, a good deal of uniformity, but this uniformity is not due so much to heredity as to prevailing social custom and convention. Consequently, when evoked by objects and events of common life, these tendencies to emotional expression developed in us by social experience carry with them a degree of social interpretation and evaluation of the situation that evoked them.[7] I remember very well an occasion years ago

[6] *Ibid.*, 255.

[7] Woodworth writes: "There is a language of the emotions, composed of gestures and postures, of exclamations and inflections and tones of the voice, of facial expressions. This language is no doubt based on the unlearned expressive movements, but it has become standardized through the long ages and now is largely a matter of social custom and convention. The child finds this language in use and appropriates it to some extent." R. S. Woodworth, *Psychology*, 3d ed., 1934, 342.

when I was instrumental through an act of atrocious carelessness and clumsiness in doing great damage to an article I prized very highly, damage I could not afford to have repaired. As I surveyed the results of my handiwork I gave expression to my overwhelming exasperation with myself, in strong language, facial expression, and expressive bodily movement. On looking back upon my emotional outburst a few hours later, when calmness had returned, I was interested and amused to recall that I went through the motions of tearing my hair, beating my breast, and wringing my hands. Reflecting upon this behavior two things are clear. First, that it was not determined by an hereditary predisposition but by accepted conventions of dramatic portrayal and literary description; and secondly, that it carried with it, by implication, an interpretative judgment upon my act from the social point of view, as not only disastrous in itself, but also as discreditable to me and deserving of censure and punishment.

Suppose it to be true that these three types of determining tendency do give social significance to the objects and events which evoke them; —what then? Does this not confuse rather than clarify the issue as to what distinguishes the social from other kinds of intelligent activity? Surely, it may be said, words are frequent accompaniments of our perceptions; the understanding of, and attitude toward, the situations we meet are continually modified by social custom and tradition; conventional emotional expression is frequently elicited by the incidents of daily life. The upshot of the discussion thus far would therefore be to show that all our responses are psycho-social, rather then to make plain what it is that distinguishes the social from all other responses. Now if the intention had been to cite the efficacy of these determining tendencies in imparting social significance to the occasions which evoke them, as an explanation of what is distinctive of the social response *per se,* this objection would be quite in order. But this was not the intention. The distinctive feature of 'social' perceiving is that, as Bentley contends, it implies the existence of a fellow man present or assumed to exist elsewhere. Not, we agree, that the mind of my fellow operates instead of the objectively verifiable facts of sense-stimulation in evoking a direct acknowledgment from my mind in independence of objectively-existent bodily tendencies; but that sense-stimuli, produced directly or indirectly by the behavior of others, evoke responses from determining tendencies which have social significance not only in the general way in which socially prevalent interpretations, opinions, interests, conventions, and valuations are reflected in our speech, in what we observe and recall, in the ways we express our feelings, but also definitely and directly, as the implication of what Bentley

calls the "plurality of observers." It is this implication, not as a logical inference from, but as a part of the concrete meaning of, the perceived object or situation, which is the distinctive meaning of the social response.

When now, with this made clear, we do undertake to explain the psychology of this response in its distinctive character, the facts we have been considering as to the main types of determining tendency which give social significance to the responses they govern, prove to be far from irrelevant.

Suppose that we begin with our response to a fellow being present in the flesh. We know that the objects to which we respond are furnished by his behavior. His behavior takes the form of outwardly-directed action, verbalization, facial expression, gesticulation, and posturing. To these sources (as to impersonal objects and events) we respond and our responses are governed by habits of speech, of social and occupational adjustment, and of emotional expression; tendencies which give, as we have seen, social significance to the responses they determine. But in the response of face-to-face human contact, another and a new factor enters the situation. In this case not merely are objects and events responded to by modes of speech, action, and emotional expression, which give them social meaning, but *the objects and events responded to are themselves the words, actions, and feeling-expressions of another which already have such meaning for him.* This—the significance which another's behavior in its different modes has for that other himself—is grasped by the agent in a response of social apprehension or understanding.

In this response the other's behavior is perceived to signify not merely his present existence, but also those beliefs, opinions, or observations which he is expressing by his words, what he is aiming at by his actions, and what emotions register themselves on his face and in his deportment. The response by which the agent perceives what his fellow is thinking, feeling, and doing is determined by his own speech-habits, customary aims and procedures, usual ways of expressing his feelings. But such response would not be possible, could not be successful, except for a further fact, the fact, namely, that these determining tendencies are shared by his fellow (as by other human associates) and are equally effective in determining his fellow's behavior. Thus we reach the fundamental condition on which the distinctively social response depends: a world of intelligible discourse, an organized system of industrial, political, domestic and cultural activities, a complex of patterned feeling-signs, common to, and shared by, all members of the social group.

This is that "frame of community" to which Dodge refers as the

essential and underlying fact of social life and relations. When I note the movements, listen to the speech, look into the eyes, notice the changing shades of facial expression of another human individual, what I really observe is a fellow being to whom the objects of the external world have the same characteristics and relations, arouse like desires, have to be dealt with by the same practical skills, stir similar emotions, that they have for, or do with, me. This community, or communication, of experiences which alone makes it possible for human individuals to understand one another, to coöperate in common pursuits, to feel for one another an appreciative, if discriminating, sympathy, this is the underlying and real basis of human association.[8]

The distinctively social response is evoked not merely by the behavior of others, but by objects and events which imply their existence. But here again this social implication is not the general one which all or nearly all objects have for the intelligence that is socially trained, hence versed in the arts of speech and the customs, manners, and standards of organized social life. It is the implication of another, or of others, existing in some relation to oneself. This implication may be 'presentative,' to use Bentley's term, as when the meal prepared and table set for two implies the guest, companion, or partner who is dining with me. It may be 'inferential' as when the object implies the fellow being who, though absent, is understood to be its source or author, maker, or creator.

This kind of social significance principally attaches, so far as I can see, to three classes of objects. There is, first, written or printed discourse which expresses to one who reads the ideas, opinions, arguments, and conclusions, of its author. Here the social implication is definite and unmistakable, though varying in degree of directness from that possessed by a personal letter to that of a circular letter addressed by an executive to all members of his organization or by a sales-manager to a list of prospective customers, and from that possessed by a signed editorial or columnist's daily output of piquant personal comment to that of a book of scientific information or a record of current events. Secondly, there is the class of objects and devices of human manufacture too numerous and varied for description which in their design, workmanship and utility imply the individuals who made them or, it may be, who now own and employ them. Here too the implication exists in every degree of definiteness. That set of shelves I had made for the corner of the room will

[8] For more extended argument that *community* in the sense explained above is the psychological basis of human association, see my article, The basis of human association, *J. Philos., Psychol., & Sci. Methods,* 17, 1920, 421-430.

always imply to me the carpenter whose skill it exemplifies, while the chair in front of it, obviously machine-made to standard pattern, signifies its makers no more definitely than the pavement outside implies the particular workmen who laid it. The third class consists of products of fine art, pictures, musical compositions, architecture, and the like. With these objects, as with those of the first class, the social implication is direct and inescapable. The object implies its creator because it communicates something of the intensity and significance of the emotion he felt and sought to express.

STIMULUS-DURATION AND SENSATIONS OF WARMTH

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It has been shown that varying stimulus-temperatures[1] and pressures[2] within certain ranges varies the number of warmth and heat sensations reported within a limited area of skin. The present paper gives the results of an experiment in which the stimulus-duration was varied systematically while other factors of stimulation, such as temperature, pressure, and size of stimulus, location and temperature of skin stimulated, and room temperature, were held constant or controlled.

Besides showing the rôle of stimulus-duration in temperature sensitivity, this study has some bearing upon theories of temperature sensation. Weber[3] and Hering[4] believed, and the opinion is generally held, that an adequate stimulus for warmth raises the temperature of the warmth receptors. Assuming a constant stimulus-temperature, a certain time should be required for raising the temperature of the sense organs; stimulus-durations below this time should fail to arouse sensations of warmth while longer durations should be adequate. If the sense organs for warmth are situated at different depths in the skin, short stimulus-durations may stimulate superficial receptors but longer durations would be necessary to produce heat conduction to the deeper receptors. Thus one might expect increasing reports of warmth with increasing stimulus-duration.

This study has also some bearing on the moot question of specialization of end-organs. Von Frey[5] suggested that the Ruffini end-organs mediated sensations of warmth. This opinion has recently been supported by the work of Bazett and McGlone and their collaborators on the thermometric conductivity and the identification of sense organs in the human prepuce.[6] In their histological study of 1 sq. cm. of the prepuce one Ruffini end-organ was found at a depth of approximately 0.286 mm. The thickness of the

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[1] F. Heiser, Stimulus-temperature and thermal sensation, *Arch. Psychol.*, 21, 1932, (no. 138), 40 ff.

[2] F. Heiser and W. K. McNair, Stimulus-pressure and thermal sensation, this JOURNAL, 46, 1934, 583 ff.

[3] E. H. Weber, Tastsinn und Gemeingefühl, Ostwald's *Klassiker der exakten Wissenschaften*, 149, 1905, 1-156.

[4] E. Hering, Grundzüge einer Theorie des Temperatursinns, *Sitzungber. der k. Akad. Wissenschaften*, Math.-Naturw. Cl., 75 (Pt. 3), 1877, 108 ff.

[5] Max von Frey, Physiologie der Haute, in J. Jadassohn's *Handbuch der Haut und Geschlechtskrankheiten*, Bd. 1, T. 2, 1929, 104.

[6] H. C. Bazett, B. McGlone, R. G. Williams, and H. M. Lufkin, Sensation, *Arch. Neurol. & Psychiat.*, 27, 1932, 508 ff.

epidermis was 0.028 mm. Maximov states that the thickness of the dermis in the prepuce is approximately 0.6 mm.[7] Therefore the Ruffini organ must have lain in the deepest part of the papillary layer or between the papillary and reticular layers of the dermis. Maximov agrees that the "so-called brushes of Ruffini . . . occur principally in the papillary layer of the skin of man."[8] Ruffini,[9] however, writes that these end-organs, which he calls corpuscles, not brushes, are numerous throughout the depth of the adipose membrane but usually in the most superficial part. He also found a few in the deepest part of the reticular layer of the dermis.

Cajal agrees with Ruffini that the Ruffini organs (called "terminal cylinders of Ruffini" by Cajal) are found in the subcutaneous connective tissue, especially in the adipose layer.[10]

Bazett and his collaborators used metal stimuli for warmth at 40° C. with a stimulating surface of 1.5 mm. diameter.[11] They report the conduction of warmth through the prepuce at the mean rate of 0.975 mm. per sec.[12] On the basis of the rate of conduction and the reaction-time of warmth they estimate the depth of the receptors of warmth at about 0.3 mm.[13] which agrees very closely with their histologic finding of a depth of 0.286 mm. for the Ruffini organ.

The present writer has worked largely upon the skin of the forehead where stimuli of certain temperatures and pressures have elicited many sensations of warmth.[14] According to Maximov's measurements of the thickness of the epidermis and dermis on various parts of the body, the skin of the forehead varies between 1 and 2 mm. in thickness.[15] Therefore, the Ruffini organs, if they are in the subcutaneous adipose layer, as stated by Ruffini and Cajal, would be at least 1 mm. below the surface of the skin.

Bazett and McGlone[16] found that at a depth of 0.95 mm., with stimulation lasting 7.4 sec. at 40.8° C., (12.2° above skin temperature) the temperature change began after 2 sec. of stimulation and increased 0.05° in ½ sec., 0.2° in 1 sec., 0.4° in 1½ sec. and 0.6° in 2 sec. In the present experiment the stimulus-temperatures used were 39° and 43° (6° and 10° above skin temperature) and the stimulus-durations were ½, 1, 2, and 4

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[7] A. A. Maximov, *Textbook of Histology*, 1930, 411.
[8] *Ibid.*, 280.
[9] G. Ruffini, Les Expansions Nerveuses de la Peau chez l'homme et quelques autres mammifere, *Rev. gen. d'histol.*, 1, 1905, 422 ff.
[10] S. Ramon-Cajal, *Histology*, 1933, 341.
[11] *Op cit.*, 508. [12] *Ibid.*, 497. [13] *Ibid.*, 510.
[14] F. Heiser, *op. cit.*, 57-67; F. Heiser and W. K. McNair, *op. cit.*, 582-587.
[15] *Op. cit.*, 407, 411.
[16] H. C. Bazett and B. McGlone, Studies in Sensation, *Arch. Neurol. & Psychiat.*, 27, 1932, 1057.

sec. It seems possible, according to the above results of Bazett and McGlone, for the 4 sec., and perhaps the 2 sec., stimuli to conduct heat rapidly enough to the depth of 1 mm. to elicit warmth but not for the 1 and ½ sec. stimulus-durations. One would, therefore, expect responses of warmth to the longest durations and none to the shortest, even if the Ruffini organs are less than 1 mm. below the surface. The results, however, do not fulfill this expectation.

Method and Procedure

Apparatus. The apparatus was the same as used and described by Heiser and McNair.[17] It controlled the temperature, pressure, duration, size and location of the stimulus. A simple copper-constantan thermocouple[18] providing an accuracy within 0.03° was used for measuring skin temperature of the areas explored.

Observers. There were 3 *Os*, two undergraduates in psychology (*P* and *M*) and the writer (*H*), all of whom had had considerable training and experience in work of this sort. Twenty-four areas of 30 spots each were used on the forehead of *P*, 5 areas of 100 spots each were explored on *M* and on *H* 1250 spots were stimulated, 750 in areas of 30 spots each and 500 in areas of 100 spots each.[19]

Method. An ink-dot was placed upon the skin of the forehead to serve as a point of reference for the upper left corner of the area to be explored. The areas were of two sizes; 49 contained 30 spots in 5 rows of 6 each and 10 contained 100 spots in 10 rows of 10 each. The first stimulation was given in row 1, along the top of the area, to spot 1, just to the right of the ink dot. The 6 or 10 spots in the row were stimulated serially and then the stimulator was moved down to spot 6 or 10 in row 2 which was explored from right to left. The remaining rows were explored in the same manner. Reports to each stimulation were given at the end of each stimulation by raising the fingers of one hand and were recorded in corresponding locations on mimeographed maps of the areas. One finger was raised to report a sensation of pressure only, and 2 fingers indicated the sensation of warmth.

Four stimulus-durations were used: ½, 1, 2, and 4 sec. The interval between stimulations was varied between 6 and 9 sec., according to the stimulus-duration. Five minutes were required to explore a 30-spot area and between 14 and 17 min. were required for one exploration of a 100-spot area. After the first exploration all areas of 30 spots were immediately reëxplored in the same way with the same duration of stimulus. Thus about 5 min. elapsed between successive stimulations of the same spot with the same stimulus. A 5-min. rest was given before the area was reëxplored with a stimulus of different duration. The 30-spot areas were explored twice to give a measure of the sensitivity of the areas to repeated stimulation after a 5-min. interval. The 100-spot areas were explored only once at each of the 4 stimulus-durations. The order of using the different durations was varied haphazardly at every experimental session.

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[17] *Op. cit.*, 580 f.

[18] Described by F. G. Benedict, Measurement of the skin temperature of humans, *Leopoldina, Amerikaband*, 4, 1929, 129.

[19] 'Spots' refers to points on the skin which were covered by the stimulator, not to specialized receptors within the skin or points on the skin where specific sensations were elicited. The stimulus had a diameter of 1.1 mm.; consequently the spots were approximately of the same size.

At each stimulus-duration the temperature of the area was taken just before and after each exploration. These skin temperatures were taken to find the effective stimulus-temperature and to show any effect of stimulation upon the temperature of the area explored.

The stimulus temperature was kept as close to 39° as possible to avoid heat sensations which are frequently elicited at higher temperatures and to insure a more constant effective stimulus temperature.[20] For 408 explorations, 8 in each of 49 30-spot areas and 4 in each of 4 100-spot areas, the mean stimulus-temperature was 39.32°, S.D., 0.34°. Six 100-spot areas were explored at an average stimulus-temperature of 42.97°, S.D., 0.1°. For the duration of the whole experiment the mean room-temperature, recorded every 5 min., was 25.94°, S.D., 1.22°. The factor of skin-temperature could not be controlled beyond the precaution of controlling room-temperature and working at the same hour of the day. Heiser and Cohen found that forehead temperature varied only slightly between 9 A.M. and noon.[21] This experiment with the exception of the 10 100-spot areas,[22] was done between these hours to have the least possible natural changes in forehead temperature during the course of stimulation. The measurements of skin-temperature before and after each period of areal stimulation were averaged to indicate the approximate temperature during each exploration. The range of skin-temperatures over the whole series of experiments was small. The mean skin-temperatures for the 3 *O*s were: *P*, 34.28°, S.D., 0.35°; *M*, 34.25°, S.D., 0.38°; and *H*, 33.76°, S.D., 0.59°. The mean effective stimulus-temperatures, then, were 5.04° for *P*, 5.07° for *M* and 5.56° for *H*. *H* reported the greatest number of warmth sensations but it is doubtful, of course, if his greater sensitivity is due to the slightly greater effective stimulus-temperature in his case.

The stimulus-point was of brass and had a diameter of 1.1 mm. The stimulus-pressure was adjusted to 2 gm.

RESULTS

As in the previous experiments there was much variability in the sensitivity of different areas under the same factors of stimulation.[23] The stimulus-temperature of 39° proved to be too low to elicit many warm sensations on the foreheads of the 3 *O*s though some *O*s have been shown to give many warmth reports at this temperature.[24]

In the following tables warmth reports are treated as percentages of the total number of spots stimulated on the forehead at the different stimulus durations. For example, 720 spots were stimulated on *P* at the duration of ½ sec., and 32, or 4.4%, were warm.

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[20] The effective stimulus-temperature is found by subtracting the skin-temperature from the stimulus-temperature.

[21] F. Heiser and L. H. Cohen, Diurnal variations of skin temperature, *J. Indust. Hyg.*, 15, 1933, 245 f.

[22] The 100-spot areas were explored between 2 and 5 P.M. during which time there is a slight increase in normal forehead temperature.

[23] Heiser, *op. cit.*; Heiser and W. K. McNair, *op. cit.*

[24] Heiser, *op. cit.*, 57-67.

Effect of stimulus-duration. In the whole series of experiments, 1870 spots were stimulated at each of the 4 stimulus-durations with a stimulus-temperature of 39°, and 600 spots were stimulated at 43° for the same durations. Table I shows the warmth reports to the first stimulations at the different stimulus-durations, showing that relatively few warmth reports were given at any duration, that the differences between the reports at the

TABLE I

PERCENTAGE OF SPOTS REPORTING WARMTH AT DIFFERENT STIMULUS-DURATIONS

O	Stim.-temp.	No. spots stimulated	Stimulus-duration in sec. ½	1	2	4
P	39°	720	4.4	3.3	5.0	3.2
M	39°	200	4.0	5.0	5.5	5.5
H	39°	950	3.6	4.8	5.6	5.5
M	43°	300	4.0	7.3	7.3	4.7
H	43°	300	13.0	14.0	14.0	15.0

different durations were small, though perhaps not insignificant and that in only 8 of the 15 cases of increased duration the warmth reports increased. The duration increase from ½ to 1 sec. was the most effective as 4 of the 5 *Os* showed an increase in the number of the reports of warmth, though the increase seems significant in only two of these cases: *H* at 39° and *M* at 43°.

Effect of the first and second stimulations. Table II compares the reports of warmth in the first and second stimulations, occurring 5 min. apart, in areas explored at 39°. Within the conditions of this experiment, restimulation after an interval of 5 min. results in about the same number of warm sensations as the first stimulations, though in 5 of the 8 comparisons there

TABLE II

PERCENTAGE OF SPOTS REPORTING WARMTH TO THE FIRST AND SECOND STIMULATIONS

O	No. spots stimulated	Stimulation	Stimulus-duration in sec. ½	1	2	4
P	720	1st.	4.4	3.3	5.0	3.2
		2nd.	4.0	4.4	4.6	5.4
H	750	1st.	3.1	4.5	5.7	5.9
		2nd.	2.8	4.0	5.6	6.4

were fewer reports to the second stimulations than to the first. Heiser and McNair[25] also reported a slight decrease of warmth reports to the second stimulations at a duration of 2 sec., pressure of 2 gm. and temperature of 43°, though the writer[26] found a slight increase in the reports of warmth in the second stimulations at 39°, 2 gm. pressure, and 2

[25] *Op. cit.*, 586-587.

[26] *Op. cit.*, 46.

sec. duration. In the latter two experiments the interval between the first and second stimulations of identical spots was from 26 to 28 sec.[27]

During the course of stimulation, the stimulus- and skin-temperatures usually increased, causing small differences between the effective stimulus-temperatures of the first and second explorations. In 6 of the 8 comparisons of reports in Table II, a change in the number of the reports of warmth accompanies a change in the opposite direction of the effective stimulus-temperature. Apparently there is a negative correlation between reports of warmth and effective stimulus-temperature but the differences in warmths and temperatures are too slight to give the correlation much significance. There is no correlation between stimulus-temperature and reports of warmth or between skin-temperature and those reports.

Comparison of the first and last thirds. With all the 30-spot areas which were explored at 39°, the skin- and stimulus-temperatures were recorded at the beginning and end of each exploration.[28] Both were usually higher at the end of an areal exploration than at the beginning. The data were, therefore, analyzed to find if there were any relation between the number of warmths in the first and last thirds of the areas and the effective stimulus-temperatures for those parts of the areas.

The first and last thirds of the areas include the first and last 10 spots respectively to be stimulated within the area. The first 10 spots were stimulated with approximately the same effective stimulus-temperature as at the beginning of exploration and the last 10 spots were stimulated with approximately the effective temperature recorded at the end of the exploration of the area.

Considering the first explorations only, *P* had mean effective stimulus-temperatures at the beginning and end of explorations of 4.9° and 5.08°. His warmths likewise increased from 4.17% in the first thirds to 5.0% in the last thirds showing a slight increase of warmths with the slight increase of effective stimulus-temperature, a relationship which is just the reverse of that shown in Table II. With *H*, the effective stimulus-temperature increased also, from 5.46° to 5.63°, but the mean number of warmths for the first and last thirds decreased from 6.1% to 4.7%.

Discussion

The present study fails to demonstrate a reliable difference between different stimulus-durations in eliciting sensations of warmth. The 4-sec.

[27] These intervals are sufficiently long to insure reliable and consistent results. Cf. R. H. Earhart and K. M. Dallenbach, The response of warm spots under successive stimulation, this Journal, 45, 1933, 722-729.

[28] For the method of accomplishing this see Heiser and Cohen, *op. cit.*, 245 ff.

stimulus-duration aroused slightly more warmths than the ½-sec. duration in 2 of the 3 *Os* (see Table I), but the difference is small and is probably not significant. This means either (1) that the receptors of warmth are so close to the surface that they are stimulated by the shortest as well as the longest durations, which rules out the Ruffini organs as the receptors, or (2) that the receptors, if they are the Ruffini organs, which according to Ruffini[29] and Cajal[30] and Maximov[31] lie at a depth of 1 mm. or more, are not stimulated by conduction of heat directly from the stimulator. The ½-sec stimulation at 39° could not raise the temperature of sense organs 1 mm. deep within the reaction-time of our reports of warmth which, it will be recalled, were returned about 1 sec. after the application of the stimulator.[32] Bazett, McGlone, and Brockelhurst applied stimuli 9° C. above skin-temperature for 6 sec. and found an increase in surface temperature of the spot stimulated of only 3°.[33] Any increase in the temperature of the sense organ at a depth of 1 mm would more likely be due to an increased circulation of blood because of the mild excitation. Bechterew writes that "every exitation of any importance produces local reaction in the region of the receptor-organ. This is expressed at times by a positive reaction of the circulation . . . and at times by a negative reaction. The vascular phenomena depend entirely upon the strength of the excitation. One may take as a general rule that very weak excitations have a negligible effect upon the vessels. The moderate excitation being favorable to the organism and stimulating to its functions is accompanied by a sthenic reaction."[34] "The thermal energy affects the cutaneous and mucous tissue. The nature of the action is as yet little known, but in all probability it is a mechanical action on the terminations of the nerves produced in the one case by the flushing and in the other case by the constriction or contraction of the tissues."[35] According to Bazett and McGlone, it is probable that the end-organs for warmth respond to a rise in temperature in their neighborhood or to a gradient in which the blood is warmer than the end-organ. However, the release of stasis in a limb immersed in water at blood temperature gives a marked sensation of warmth, even though the blood enters tissue of the same temperature and produces no measurable thermal gradient nor any change in temperature. The actual stimulus would there-

[29] *Op. cit.* [30] *Op. cit.* [31] *Op. cit.*

[32] Bazett and McGlone (*op. cit.*, 1057) found, with a more intense stimulus than ours, that no temperature-change occurred at depths of 0.95 mm. until 2 sec. after stimulation.

[33] H. C. Bazett, B. McGlone, and R. J. Brockelhurst, The temperatures in the tissues which accompany temperature sensations, *J. Physiol.*, 69, 1930, 88-112.

[34] W. Bechterew, *La Psychologie Objective*, 1913, 90.

[35] *Ibid.*, 43.

fore appear to be a chemical, not a thermal, gradient.[36] Von Frey[37] said that the sensation of warmth is caused by a flow of blood into comparatively warm skin.[38]

If the Ruffini organs are the true receptors for warmth, it is certainly doubtful, then, that they are stimulated by heat flow directly from the stimulator at the surface. Thus the Weber and Hering theories are questioned and Ebbecke's theory[39] that temperature sensations are induced by alternations of spacial thermal gradients, due chiefly to circulatory factors, is favored.

The assumption that Ruffini organs mediate sensations of warmth should be subjected to much more conclusive experimental evidence in consideration of the well known failure to identify specialized receptors beneath warmth and cold spots.[40] In view of the probable importance of vascular factors in temperature stimulation, the negative histological results should not deny specific receptors as they need not be located directly beneath the spot stimulated on the surface or within the depth of histologic sections which have thus far been made.

Summary

Forty-nine areas of 30 spots each and 10 areas of 100 spots each were explored on the foreheads of 3 experienced *O*s with a mechanical, cutaneous, temperature-stimulus. The stimulus-durations were varied systematically while other factors of stimulation were held constant.

The procedure in the 30-spot areas was such as to give each spot two applications of the same stimulus at an interval of 5 min. The correspondence between the reports from single spots and areas was high. The 100-spot areas were explored only once.

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[36] Bazett and McGlone, *op. cit.*, 1067 f.

[37] M. von Frey, Physiologie der Sinnesorgane der menschlichen Haut, I. Der Temperatursinn, *Ergeb. d. Physiol.*, 9, 1910, 351 ff.

[38] If the foregoing hypotheses are true, it would seem useless to guess at the depth of sense organs for warmth on the basis of reaction-times. T. Thunberg, Untersuchung über die relativ Tiefenlage der Kälte-, Wärme- und Schmerz percipirenden Nervenenden in der Haut, und über das Verhältniss der Kältenervenenden gegenüber Wärmereizen, *Skand. Arch. f. Physiol.*, 11, 1901, 382-435. A. Sütter, Die Unterschiedschwellen des Temperatursinnes, *Zsch. f. Biol.*, 74, 1922, 237-298.

[39] U. Ebbecke, Über die Temperaturempfindung in ihrer Abhangigkeit von der Hautdurchblutung und von der Reflexzentren, *Arch. f. Physiol.*, 169, 1917, 395 ff., 427 ff.

[40] H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 408-416. A. Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerves beim Menschen, *Arch. f. Anat. u. Physiol.*, Physiol. Abt., Suppl. Bd., 1886, 191-227. Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 45, 1913, 46-63. K. M. Dallenbach, The temperature spots and end-organs, this JOURNAL, 39, 1927, 402-427. C. R. Pendleton, The cold receptor, *ibid.*, 40, 1928, 353 ff.

In general, the increase in stimulus-duration did not produce a significant change in the number of sensations of warmth within the areas.

Practically the same number of warmth sensations was elicited by the first and second stimulations and in the first and last thirds of the areas.

The surface temperatures of the areas of skin stimulated rose slightly during the period of stimulations.

Although the adequate stimulus for the sensation of warmth may be an increase in the temperature of the sense organ, the increase is probably not caused by conducted heat from the stimulus but may be due to circulatory reactions.

PRACTICE EFFECTS ON VARIOUS TYPES OF STANDARD TESTS

By EDWARD B. GREENE, University of Michigan

The results of practice on standard test forms are important both in theoretical and applied psychology. In the theoretical field the qualitative and quantitative changes which take place during practice would, if carefully evaluated, contribute much information about the nature of learning and help in a systematic explanation of behavior. In the applied field, the prediction of success in vocational and educational activities would be greatly improved if individuals could be compared at known levels of practice. For predicting future developments, it is seldom useful to compare two individuals when it is known that one of them has had considerably more practice than the other.

PROCEDURE

This article shows in part the results of repeating certain standard tests 4 times with fairly equivalent groups of college students at intervals of one day. It reports the changes of certain raw measures during practice, and indicates possible improvements in the technique of interpretation.

The students were 253 college sophomores, men and women, at the University of Michigan. Each student spent only ten hours on this work; hence it was not possible to give all the students all the tests. They all took the Vocabulary, Speed of Reading, and Michigan Non-Verbal Series. On the basis of these test scores, smaller groups were chosen, which had the same range and median as the whole group. The groups compared are therefore nearly equivalent in this respect.

The work was done in a quiet class room in the late afternoon with from 25 to 30 students in a group, except the individual tests, which were administered singly. The students were apparently well and constantly motivated. In general, they found the work interesting, and they desired to surpass their own previous scores, of which, however, they were in ignorance. They were a highly selected sample, with a mean age of 20 yr. and 5 mo., S.D. 7 mo.

The tests were selected to give a wide range of difficulty in fields of sensory discrimination, processes using language and number symbols, processes using visual, auditory, and kinesthetic discrimination, and motor coördination. These tests, as they are customarily described, are listed in Table I. This list of tests was limited by the costs of administration and of materials, but it contains samples of nearly all the commonly used types of tests.

Four types of scores were encountered.

(1) Amount recalled, without time limits: vocabulary,[1] auditory digit span,[2] tonal memory.[3]

* Accepted for publication November 12, 1935.

[1] Edward B. Greene, A vocabulary analysis project (unpublished). Ten equivalent forms using definitions are available.

[2] *Ibid.*, Auditory digit spans at the college level (unpublished). Five equivalent forms, using from 6 to 20 digits, were available.

[3] C. E. Seashore, *The Psychology of Musical Talent*, 1919, 1-288.

(2) Liminal discrimination, without time limits: pitch, time, consonance.[4]

(3) Amount completed in limited time: tapping, aiming, feature comparison, pencil maze,[5] speed of reading,[6] equation completion.[7]

(4) Time required to complete a task: Kohs' cube,[8] Stenquist mechanical appliances,[9] Minnesota spatial relations.[10]

In order to make these scores more comparable, the fourth type of score was changed to the third by taking the reciprocal of the time score for each task.

The standard directions for administering these tests were used with the exception of the Kohs' cube and the Stenquist mechanical aptitude tests, whose time limts were so short that some *S*s could not complete all the items. In these cases, the actual time needed to complete each task was recorded for each *S*, then transmuted into an amount-per-second score. These scores are used hereafter in Table I.

It is obvious to one acquainted with these tests that scores of types *(1)* and *(2)* used items which were progressively more difficult, whereas types *(3)* and *(4)* used items which were nearly equivalent. Thus the score on the vocabulary test depends upon knowledge of words which are progressively more rare or complex, but the scores on an aiming, tapping, form board, or cube design test depend, in this study, upon repeating the same or very similar tasks in the shortest possible time. Furthermore, many large and obvious differences in process can be easily pointed out between various tests which have the same type of score. There are two methods for comparing the increases of these various types: the S.D. units of Thurstone,[11] and the isocron units of Courtis.[12] Both of these methods equate items for difficulty, not in terms of complexity or similarity of processes but in terms of the proportion of persons in a normal group who are able to pass each item. These methods cannot be applied to this material because of the small selected samples used.

The averaging of raw scores as in this article, is not justified for the most careful work, but it shows the main tendencies fairly adequately. A still *more* thorough approach to the problem must be made in the future by comparing individual practice curves for similar tendencies.

Results

The results are given here in two forms: first the numerical; and secondly the qualitative. The numerical results show amounts of change in central tendency, dispersion, percentage of increase, S.D. of increase, and

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[4] Seashore, *op. cit.*

[5] Edward B. Greene, The Michigan non-verbal series, *J. Soc. Psychol.*, 3, 1932, 354-359. (Four equivalent forms were used.)

[6] E. H. de Weerdt, A study of the improvability of fifth grade school children in certain mental functions, *J. Educ. Psychol.*, 18, 1927, 547-557.

[7] John C. Peterson, *Equation Completion Test*, 1928. (One form used, divided into four parts. Ten minute periods.)

[8] S. C. Kohs, *Measurement of Intelligence*, 1927.

[9] J. L. Stenquist, *Mechanical Aptitude Tests*, 1922. (One box only used.)

[10] D. G. Paterson, Minnesota mechanical ability tests, *West. Arts Assoc. Bull.*, 14, 1930, 95-98.

[11] L. L. Thurstone, Scale construction with weighted observations, *J. Educ. Psychol.*, 19, 1928, 441-454.

[12] S. A. Courtis, Maturation units for the measurement of growth, *School & Soc.*, 30, 1929, 683-690.

coefficient of variation, in Table I. The changes in central tendency are shown also in Fig. 1. An inspection of this table shows that the tests group themselves according to relative amounts of increase with practice.

TABLE I

TESTS ARRANGED IN ORDER OF AMOUNTS OF INCREASE WITH PRACTICE

	Relative increase	
	%	S.D.
(1) Increases from 0 to 5% include:		
1.1 Liminal comparison of auditory pitch	0.1	.08
Consonance	3.3	.18
Time interval	3.8	.17
Tonal memory	.1	.05
1.2 Speed of movement in tapping, 10 sec.	4.9	.18
(2) Increases from 6% to 25% include:		
2.1 Specific information, vocabulary	12.2	.46
2.2 Accuracy of movement, as in aiming for 1 min. at 1.5-mm. circles	16.0	.88
2.3 Auditory digit span	22.0	1.34
(3) Increases from 26% to 75% include:		
3.1 Easy comparison, such as:		
Speed of easy reading	34.0	.86
Easy feature comparison	61.0	1.83
Easy pencil mazes	67.0	2.95
3.2 Hard comparisons, such as:		
Hard feature comparison	54.2	.97
Difficult cube design, Kohs' No. 14	75.0	3.00
3.3 Easy form board, such as:		
Minnesota spatial relations	59.9	3.01
(4) Increases from 76% to 200% include:		
4.1 Comparisons of medium difficulty, such as:		
Medium pencil mazes	137.4	2.39
Medium feature comparisons	157.0	3.20
Equation completion of Peterson	192.0	3.38
Kohs' Design No. 1	136.4	3.38
Kohs' Design No. 5	111.0	4.50
Kohs' Design No. 10	101.0	4.50
(5) Increases of more than 300% include:		
5.1 Tests where solutions of puzzles can be remembered, such as Stenquist Mechanical Assembly:*		
Cupboard catch	350.0	7.33
Bicycle bell	520.0	6.30
Push button	520.0	6.00
Mouse trap	1900.0	32.00
5.2 Hard pencil mazes, no specific memory	307.0	4.25

* The entire series was administered. The selected results shown here are typical of those which have been omitted.

There is a very noticeable relationship between improvement as shown by percentages of first trial scores and by standard deviations (S.D.) of first trial distributions. Thus an improvement of 5% or less corresponds

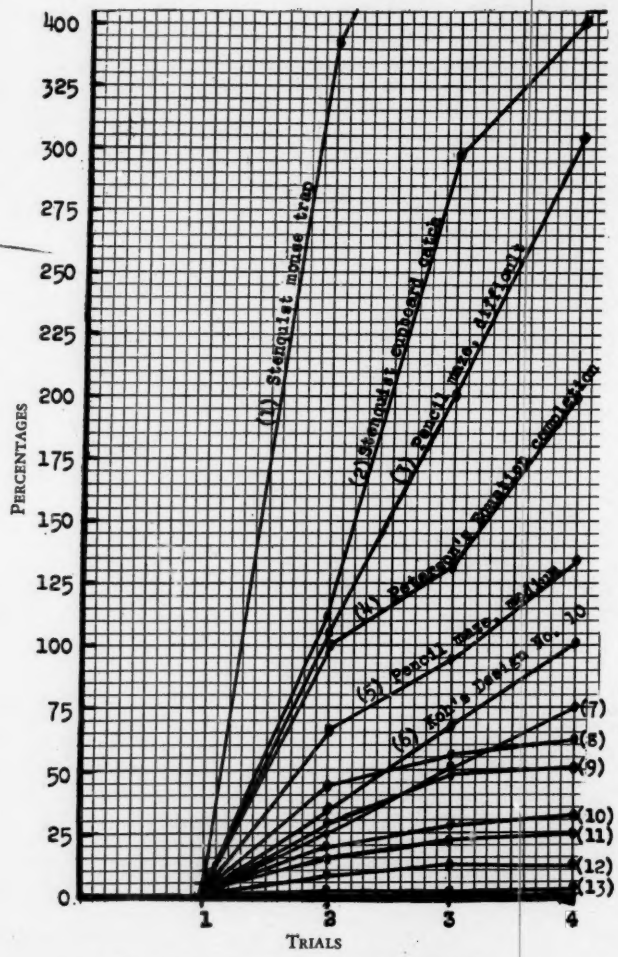

FIG. 1. PRACTICE EFFECTS ON STANDARD TESTS

Average percentage of improvement in four repetitions by 76 college students. (1) Stenquist mouse trap; (2) Stenquist cupboard catch; (3) Pencil maze, difficult; (4) Peterson's equation completion; (5) Pencil maze, medium; (6) Kohs' design no. 10; (7) Kohs' cube design no. 14; (8) Feature comparison, pencil maze, both easy; (9) Minnesota space relations; (10) Easy reading, speech; (11) Auditory digits span; (12) Accuracy, aim, vocabulary; (13) Tonal memory, tapping, time, pitch, constancy.

to an improvement of less than one-half of the S.D. Improvement from 6% to 25% is approximately the same as from one-half to one S.D. Improvement of from 26% to 75% corresponds to from 1 to 1.5 S.D., and from 75% to 200% includes from 1.5 to 3 S.D. Above 300% represents

TABLE II

SUBJECTIVE ANALYSIS OF INDEPENDENT PROCESSES

(1) Memory:
- 1.1 Memory span, or immediate recall, digits.
- 1.2 Specific preliminary information, as in the Vocabulary test.
- 1.3 General preliminary, as reading habits, methods, etc.
- 1.4 Specific information learned during the test: the way to put a bicycle bell together.
- 1.5 General information or adjustments learned during the test: good methods of approach to a problem situation.

(2) Comparison:
- 2.1 Direct liminal comparison, as in the pitch discrimination.
- 2.2 Direct supraliminal, sometimes called simple perception, as in easy feature comparison.
- 2.3 Eduction of easy relationships, as in easy reading and simple feature and maze tests.
- 2.4 Eduction of hard relationships or puzzle type of test, as in the Stenquist Mechanical.

(3) Movement:
- 3.1 Speed of movement, as in tapping.
- 3.2 Accuracy or precision of movement, as in aiming.
- 3.3 Rhythm or grouping of movements, which is found in tapping, aiming, and form boards.

(4) Persistence: avoidance of distractions, control of energy, as in long or baffling tests.

from 4 to 32 S.D. S.D. scores are not very reliable on these tests, because of the small groups and the skewing of some of the distributions.

There is an interesting relation between the relative amount of improvement and the changes of coefficient of variation with practice. The greater the relative improvement, the greater was the decrease in C.V. This indicates that the means increased relatively more than their own S.D. Thurstone has pointed out that such differences in C.V. are probably the results of arbitrary scaling.[13] They become nearly constant as the true zeros are used. Richardson[14] has suggested that the "universal C.V." for certain kinds of mental tests for normal populations is approximately 17.5. The figures in this table seem to confirm this hypothesis in a rough way, for the C.V. on the fourth trial for these small superior samples tends toward 14.5. A very large amount of careful research will need to be completed to establish the true relationship of these various sorts of trends.

[13] Thurstone, *op. cit.*

[14] C. A. Richardson, Growth and variability of intelligence, *Brit. J. Psychol., Monog. Suppl.*, 18, 1933, 1-23.

The shapes of the practice curves (Fig. 1) which showed increases are all fairly well described as negatively accelerated. More repetitions would be needed to show the tendencies adequately.

The qualitative changes which occur with practice are probably reflected in the different relative improvements of the various tests. Thus those tests which have nearly the same scores throughout, presumably (also) depend on processes which change very little with practice. Conversely, if a test score shows great improvement with practice, it is probable that various processes are eliminated or introduced. The analysis of these processes to show their nature and their interrelationship or independence will of course be the work of years of elaborate research. A classification of independent processes on the basis of the qualitative reports of 76 students and observers made during or immediately following the test periods is shown in Table II.

Probably all the tests used employ all these processes to some degree, but the scores in some cases seem very largely due to one or two types of processes. Thus Table I shows that those tests which increase from 0 to 5% include principally two types, either speed of movement or liminal comparisons. Increases from 6% to 25% include tests which depend principally upon specific preliminary information, accuracy of movement and rhythm, and direct comparison. Increases from 26% to 75% include tests of easy comparisons. Increases from 76% to 200% include principally tests where a general information or method could be learned during the tests; and increases of more than 300% included principally puzzle variety of hard eductions, whose solution led to specific information during the test situation.

The subjective analysis of practice effects on these tests also showed that with improvement, one pattern tended to become more dominant. Less trial and error, and less shifting of method on some logical basis was found after practice. In tests where much improvement was noted, the processes which dominated the test behavior on the first trial were seldom the same as those which dominated on the fourth trial. This was particularly true where a rote memory procedure was substituted for a puzzle solving activity as in the Stenquist mechanical appliances. A statistical factor analysis has been made of certain of these tests,[15] which also shows the variation of an individual's position in the group.

Practically, it seems clear that *only* those tests which show from 0 to 25% improvement can be used for comparisons of individuals. These compari-

[15] Greene, A factor analysis applied to practice effects (unpublished).

sons will be useful only when the individuals are well drilled and similarly motivated in test procedures, for it is probably true that a considerable percentage of the increases in tests of this group are due to changes in methods of approaching the test and in attitude toward the test situation. It should be kept in mind, of course, that even in these tests we are not measuring native ability alone, but native ability plus various large amounts of experience before the tests were administered. In this study, we have *no* knowledge of previous opportunities and interests. All we can claim is that the *S*'s performance is relatively more predictable on these tests than on the others.

All the other tests cannot be used for individual or group comparison until the practice effects have been somehow evaluated and removed. There seem to be two possible approaches to this problem. (1) The repetition of equivalent test forms until all *S*s have reached a high degree of efficiency or show no further tendency toward improvement. This method is extremely laborious and expensive, and may not show reliable results even under the most standardized laboratory conditions. The number of repetitions necessary will vary according to a large number of factors. (2) Another method depends upon an hypothesis suggested by Courtis,[16] that a person's ratio between his scores on two tests, which vary only in a single factor, will be constant throughout his practice and growth, and will adequately represent his ability in this particular function. A good deal of work is now being done which should prove very fruitful along this line.

Discussion

This report is in essential agreement with the reports of similar studies: first with regard to changes in standard deviations and in coefficients of variability; and secondly, in regard to the amounts of improvements.

Margineau,[17] Wright[18] Thorndike,[19] Perl,[20] Chen,[21] and Harter[22] all show similar

[16] Courtis, Maturation as a factor in diagnosis, *N.S.S.E. Thirty-fourth Year Book*, 1935, 169-187.

[17] N. Margineau, *Psihologia exercitiului*, 1929, 1-212.

[18] M. B. Wright, Development of mental ability at the college adult level, *J. Educ. Psychol.*, 22, 1931, 610-628.

[19] E. L. Thorndike, The effect of practice in the case of purely intellectual function, this Journal, 21, 1910, 483-486.

[20] R. E. Perl, The effect of practice on individual differences, *Arch. Psychol.*, 24, 1934, (no. 159), 1-54.

[21] H. S. Chen, The comparative coachability of certain types of intelligence tests, *Teach. Coll. Contrib. Educ.*, 1928, (no. 338), 1-101.

[22] R. S. Harter, A study of individual differences in association capacity, *Ped. Sem.*, 44, 1934, 139-152.

tendencies for coefficients of variability to be reduced with practice or to remain the same, while the S.D. tend to increase with practice on complex tasks. A possible instance to the contrary is Reed,[23] who found that the S.D. on achievement tests decreased during the school year. This may have been due, however, to differences in amounts of practice and motivation in the students compared.

With regard to the amounts of improvement, these same authors as well as De Weerdt,[24] and Anastasi[25] found that sensory discrimination and motor speed showed less practice effect than language and number tasks. In general the amounts of improvement correspond to a subjective estimate of the complexity of the task.

The subjective analysis of independent processes (Table II) is in some ways in accord with the statistical factor analysis of Spearman, Thurstone, and Hotelling, as described by Alexander.[26] Alexander finds only five factors. His general factor (g) is most like "comparison," and it is most noticeable in those tests which show the greatest improvement. His verbal (v) and practical factors (F), correspond to the language information and accuracy of manipulation. His persistence (X) was reported by our *Ss*, but his unnamed factor (Z) finds no counterpart in these results.

Summary and Conclusions

In the study reported here twenty-five standard tests of sensory discrimination, motor speed and coördination, and of processes using language and number symbols were repeated 4 times with groups of college sophomores. The results show a wide range of improvements, ranging from 0 to 1900%. The percentages of improvements were found to indicate rather closely improvements as shown by the S.D. of the first trial and also reductions in coefficients of variation from first to fourth trial.

Tests of speed of movement and liminal discrimination show very little improvement with this amount of practice. Tests of auditory digit span, specific information, very easy cube designs, and accuracy of movement show approximately 10% improvement. Tests which involved simple manipulation and perception, as in the Minnesota spatial relations, and the easy pencil mazes and feature comparisons show between 50% and 70% of improvement. Tests where a technique for solution could be developed by repetition, such as medium pencil mazes and Peterson's equation completion tests and Kohs' block designs, nos. 10 and 14, show between 100% and 150% improvement. Tests where a puzzle could be solved and remembered, on subsequent trials, such as the Stenquist mechanical assembly, showed more than 300% improvement.

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[23] H. B. Reed, The influence of training on changes in variability in achievement, *Psychol. Monog.*, 41, 1931, (no. 185), 1-59.

[24] De Weerdt, *op. cit.*

[25] A. Anastasi, Influence of practice on test reliability, *J. Educ. Psychol.*, 35, 1934, 321-335.

[26] W. P. Alexander, Intelligence, concrete and abstract, *Brit. J. Psychol., Monog. Suppl.*, 19, 1935, 1-29.

The relative improvements probably indicate different types of processes used and also different true zeros in the scale used.

Practically, tests which show more than 25% increase in this study are probably unreliable for comparisons of either groups or individuals. Tests which show less than 25% change with practice may prove useful when the individuals are well trained and similarly motivated.

Two possible methods of eliminating practice effects from test scores are outlined.

VISUAL DISCRIMINATION OF TWO PARALLEL BRIGHT BARS IN A DARK FIELD

By GLENN A. FRY, Ohio State University and PERCY W. COBB, Washington University

When a stimulus, consisting of two bars or stripes, separated by an interval, is presented to the eye, the blur of the optical image on the retina limits the smallness of the gap between the two bars that the eye is able to see. The gap becomes invisible, however, before the optical images of the bars actually blur together.[1] This disappearance is then not wholly explained by the optical blurring. Hartridge has proposed and elaborated in considerable detail the theory that optical blurring reduces the difference in intensity between the image of the gap and the images of the bars, and that the final disappearance of the gap results from the failure of the eye to discriminate this difference, according to the accepted proposition that the threshold is a finite, if small, quantity.[2]

In the present paper an attempt is made to apply this theory quantitatively to the effects of changing the brightness of two bright bars in a dark field upon the resolution threshold of the two bars. Other types of visual problems are related to the particular one which we have chosen to investigate, *e.g.* the resolution of two dark bars in a bright field, the resolution of a grating of dark and bright bars, and the resolution of two stars. The same kind of analysis which we are making of the resolution of two two bright bars in a dark field might be extended to these other problems, but we have not yet attempted any such extension.

Before one can interpret properly the results of manipulating stimulus-conditions upon the resolution of two bars, it is necessary to determine precisely the effect of optical blurring upon the retinal image. If one could determine the distribution of light across the blurred retinal image of an infinitely narrow bar, he could calculate from this the distribution of light across the image of two parallel bars of any width and any degree of separation. A new method has been described in a previous paper for determining the distribution for an infinitely narrow bar.[3] It was assumed

* Accepted for publication September 25, 1935. From the Department of Ophthalmology, Washington University.

[1] The participation of G. A. Fry in this investigation was made possible by a grant from the American Academy of Ophthalmology and Otolaryngology.

[2] H. Hartridge, Visual acuity and the resolving power of the eye, *J. Physiol.*, 57, 1923, 52-67.

[3] G. A. Fry and P. W. Cobb, A new method for determining the blurredness of the retinal image (in press).

that this distribution obeys the Gaussian equation and a method was devised for measuring σ. It was found for a distance of 2 m. and an artificial pupil 2.33 mm. diam. that σ is equal to 44″ of visual angle. From this it is pos-

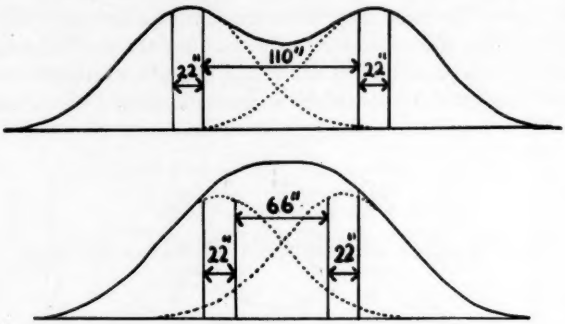

FIG. 1. EFFECT OF VARYING THE SEPARATION OF TWO PARALLEL BARS UPON THE DISTRIBUTION OF LIGHT IN THE RETINAL IMAGE
The bars subtend a visual angle of 22″.

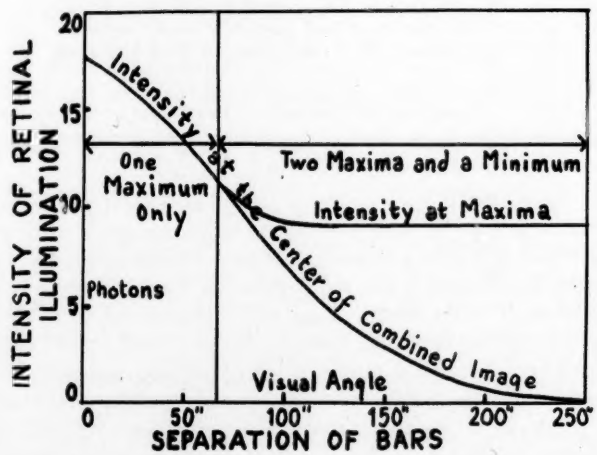

FIG. 2. INTENSITY AT THE MAXIMA AND THE MINIMUM IN THE RETINAL IMAGE OF TWO PARALLEL BARS, 22″ WIDE, FOR VARIOUS DEGREES OF SEPARATION
At 66″ the two maxima merge into one at the center of the combined image. The intensity-values are calculated for bars having a brightness of 1 c. per sq. ft. and a pupil 2.33 mm. in diam.

sible to calculate the distribution across the blurred image of two parallel bars. When two narrow bars are made to approach each other their blurred retinal images gradually merge into one. Fig. 1 shows two stages of this process for bars which are 22″ wide. The intensity increases, both at the maxima, and at the minimum; that at the minimum increases faster until it reaches that of the maxima on either side; at the same time the two maxima shift toward each other and merge into one, thus obliterating the image of the interval. The intensities at the minimum and the maxima have been calculated for different degrees of separation of the bars, and the

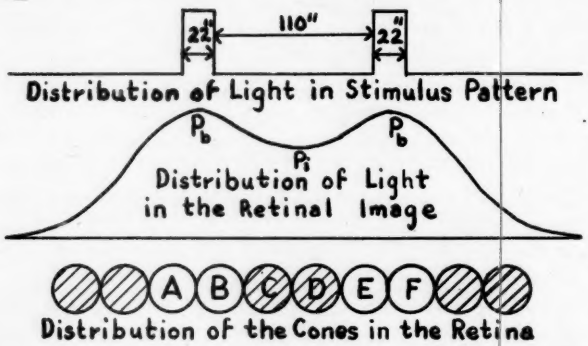

FIG. 3. RELATION OF THE DISTRIBUTION OF LIGHT IN THE RETINAL IMAGE OF TWO PARALLEL BARS TO THE DISTRIBUTION OF THE CONES IN THE RETINA
The cones are spaced at intervals of 33″ in the illustration, but the actual value may range from 28″ to 44″.

results are plotted in Fig. 2. The point is indicated at which the two maxima merge into one and obliterate the depression at the center of the image. Similar graphs can be constructed for bars of other widths.

On the physiological side of the problem, we have to ascertain how the retina responds to the blurred image of two parallel bars. In Fig. 3 is shown the distribution of light across the image of two bars 22″ wide and separated by an interval of 110″. The retinal cone responds with impulses of greater or less frequency, depending upon the quantity of light falling upon it, so that in the retina a continuous gradation of illumination is transformed into a pattern of discrete units responding with different frequencies. The physiological impression of the interval between the two bars must consist in a slower rate of response for cones C and D than for A, B, E and F. In order for the impression to reach consciousness the difference between the intensity of the image of the interval (P_i) and the intensity

of the bar-images (P_b) must exceed a certain minimum. Why the impression fails to make its way to consciousness when the difference falls below this minimum is not known.[4] The threshold difference in intensity ($P_b - P_i$) varies with the general level of P_b and P_i. According to Weber's law for brightness discrimination $(P_b - P_i)/P_i = k$; but this law does not hold at low intensities, for at such levels the value of Weber's fraction increases.

Another factor which affects the visibility of the interval indirectly is the tendency of the borders at the outer edges of the bars to interfere with

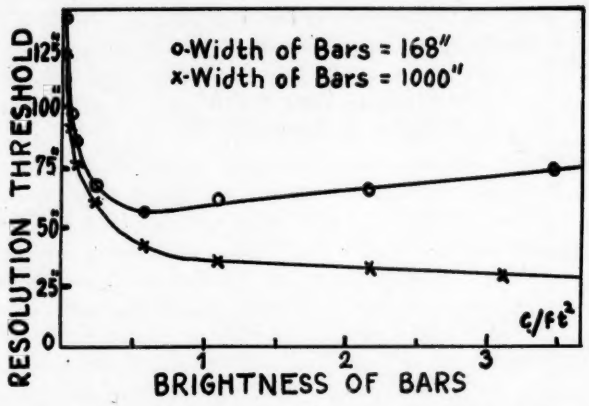

FIG. 4. EFFECT OF VARYING THE BRIGHTNESS OF TWO BARS IN A DARK FIELD UPON THEIR RESOLUTION THRESHOLD

The two curves show a comparison of the results obtained with bars of different widths. Each value represents an average of 10 readings.

the physiological impression of the interval, so that it is necessary to increase the intensity difference between P_b and P_i in order to force the impression through to consciousness. This type of phenomenon has been investigated with other kinds of stimulus-patterns by Dittmers[5] and by

[4] Evidence has been presented in a previous paper (G. A. Fry and V. M. Robertson. The physiological basis of the periodic merging of area into background, this JOURNAL, 47, 1935, 644-655, that the impression of an area with a subthreshold difference in brightness from its background fails to reach consciousness because the process subserving brightness contrast at the border fails to be established It was shown that this process is located at some level in the optic pathway prior to the convergence of the paths from the two eyes. It is not known where paths from corresponding points converge, so that the process might be either in the retina, or the lateral geniculate body of the cortex.

[5] F. Dittmers, Über die Abhängigkeit der Unterschiedschwelle für Helligkeiten von der antagonistischen Induktion, *Zsch. f. Sinnesphysiol.*, 51, 1920, 214-232.

Fry and Bartley.[6] It was shown that the interfering capacity of a border is increased by increasing the brightness difference between the two areas which it separates, and that the effectiveness of the interference depends upon the nearness of the border in the visual field to the impression which it inhibits. The demonstration that this factor plays a rôle in the resolution of two bars is one of the principal achievements of the present investigation.

We have investigated the effect of varying the brightness of two bars in a dark field upon their resolution threshold with two sets of bars, the width of the bars in one set being 1000″ and the width in the other 168″. The

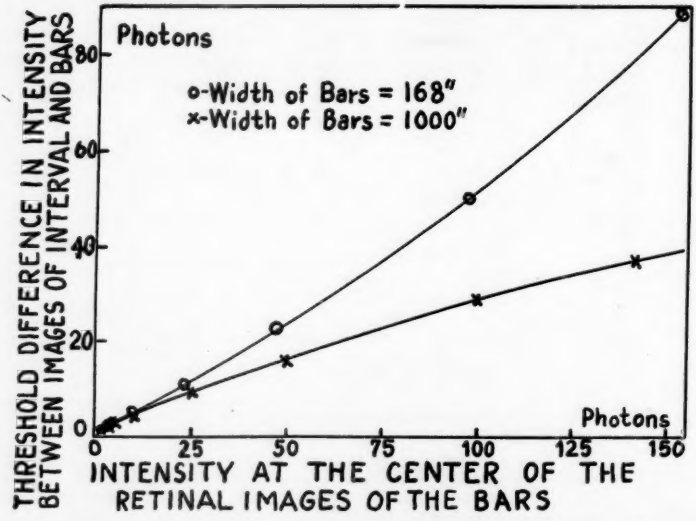

FIG. 5. DATA IN FIG. 4 REPLOTTED IN TERMS OF THE INTENSITY AT THE CENTER OF THE RETINAL IMAGES OF THE BARS AND INTERVAL

They exhibit the threshold difference in intensity between the interval-image and the bar-images for different intensities of the bar-images. The two curves show the results obtained with bars of different width. The intensity of the bar-images is determined by the width and brightness of the bars, the intensity of the interval-image by the width of the interval and the brightness of the bars.

length of the bars in both cases was 2000″. The results are plotted in Fig. 4 in terms of the stimulus-conditions, and in Fig. 5 they are replotted in terms of the intensities of the retinal images of the bars and interval. The transformation of the data was accomplished by means of graphs similar

[6] G. A. Fry and S. H. Bartley, The effect of one border in the visual field upon the threshold of another, *Amer. J. Physiol.*, 112, 1935, 414-421.

to Fig. 5. If the difference in intensity between the bar-images and the interval-image were the only factor determining the threshold of visibility of the interval, the two curves in Fig. 4 should coincide, but as P_b increases the curves separate and the separation gradually increases. This discrepancy can be attributed to the interference of the outside borders of the bars, because, in the case of the narrow bars, the interfering borders are nearer to the interval making the interference more effective and the threshold correspondingly higher. Furthermore, the capacity of the outside borders to interfere is accentuated by increasing the brightness contrast between the bars and the background. Hence at low intensities of the bars the interference is negligible but becomes more and more pronounced as the intensity goes up. Moreover, the opposite curvature of the two curves in Fig. 5 indicates that interference in the one case is not the simple multiple of that in the other.

SUMMARY

The effect of varying the brightness of two bars in a dark field upon their resolution threshold has been investigated. According to our interpretation of the results, the visibility of the interval between the two bars is determined by two factors. (1) There must be a certain minimum difference in intensity between the images of the bars and the image of the interval before an impression of the interval can be conscious. (2) The outside borders of the bars interfere with the impression of the interval and make it necessary for the intensity-difference between the images of the bars and the image of the interval to be increased.

MODIFICATION BY SUGGESTION OF SOME VESTIBULAR AND VISUAL RESPONSES

By Roy M. Dorcus, Johns Hopkins University

Phenomena such as hysterical anesthesia, anacusia, anopsia, and hypnotically induced phenomena of a similar nature are usually explained by "dissociation" or "functional modification." Such an explanation is useful in so far as it enables us to use common language in our discussions but the acceptance of such explanations lessens the probability of ascertaining what is dissociated or what functional modification really involves. In the experiments which are to be reported, an attempt will be made to show that the generally accepted hypothesis of dissociation needs to be radically modified if it is to be retained as one of the plausible theories.

Since the implications of the dissociation theory will need to be critically evaluated when the results of the experiment are interpreted, further discussion will be deferred until later in the paper. In these experiments, several responses with which most people are unfamiliar have been selected for investigation. The responses of an unfamiliar nature will afford a check on the effects of auto-suggestion which have been recognized as potent factors in dissociation. The responses that have been studied include (1) nystagmus, (2) the falling reaction, (3) pupillary reaction to light, and (4) the production of colored after-images.

(1) Nystagmus. A comparison of the post-rotational eye movements (nystagmus) resulting from actual rotation and suggested rotation under hypnosis was made by means of direct observation and motion pictures. The procedure in this experiment was to (a) rotate the *S* in the Dunlap chair; (b) to observe or photograph the actual nystagmus; (c) to hypnotize the *S* while seated in the chair and give positive suggestions of rotation, turning around, and the experiencing of feelings like those during actual rotation; and (d) to observe or photograph the eye movements.

The *S*s, 4 in number, reported when questioned that they had had a definite illusion of rotation and the feelings accompanying actual rotation. Analysis of the motion-pictures, however, confirmed the observations that the eye movements under hypnosis were of a voluntary type and were unlike the true nystagmic movements following actual rotation. The true nystagmic movement consists of a slow drift phase followed by a quick saccadic phase whereas the voluntary movements were like those accompanying reading or the examination of a series of objects. It should also be pointed out again that every one of the *S*s had actually been rotated before the hypnotic test and consequently had the opportunity of knowing the type of response which occurs following rotation.

* Accepted for publication October 21, 1936.

(2) Falling reaction. The *S*s, 6 in number, who were unfamiliar with the technique or the falling reaction that usually occurs following rotation were subjected to the following routine.

Every *S* was first placed in the Dunlap rotator and hypnotized. He was then instructed to keep his eyes closed, to place his elbows on his knees, and to rest his head in the palms of his hands. *E* further suggested that he was going to be rotated; that he would feel himself turning around and around; and that he would have all the feelings that go along with rotation. The motor was actually started to enhance the illusion of rotation, but the clutch was not engaged. After a brief period of continual suggestion of rotation, *S* was told to sit up. None of the 6 *S*s showed any imbalance, or any other sign associated with the falling reaction, although each of them uniformly admitted when questioned, that they had been rotated. Following the above procedure, they were awakened.

S was next rotated and the falling reaction obtained. After this, he was rehypnotized and the procedure outlined above was repeated. The response was secured in every case but it did not conform in direction to the suggested direction of rotation, but the *S*s did not know that clockwise rotation produces falling to the right while counter clockwise rotation produces falling to the opposite side.

(3) Pupillary reaction. The procedure for measuring the pupillary change was not essentially different from that used by other experimenters. *S* before being hypnotized, was seated before *E* facing a 40 watt lamp 18 in. from his eyes. The method of dimming or brightening the lamp by changing a resistance was demonstrated to him. A transparent celluloid strip, scaled in millimeter divisions was held directly before the pupil so that any change could be noted by *E*. Readings were secured with maximal brightness, followed by readings with minimal brightness. *S* was later hypnotized and suggestions of dimming the light were given.

TABLE I

THE DIAMETER OF THE PUPIL (IN MM.) OF NORMAL AND OF HYPNOTIZED Ss

S	Normal		Hypnotized	
	Bright light	Dim light	Bright light	Suggested dim light
A	3	5	3	3
B	2	4	2	3
C	3	6	3	3
D	3	7	3	3
F	2	7	2	2
G	3	5	3	3

The rheostat was moved in the proper direction for dimming but a short-circuiting switch prevented any actual change. The results (see Table I) of this experiment show that no appreciable effect was produced by the hypnotic suggestion of dimming the light. Every one of 5 *S*s showed an increase in pupillary size when the light was dimmed that ranged from 2–5 mm., whereas not one of the *S*s showed an increase for the suggested dim light.

(4) After-image. Cards 3b, 4a, and 6c of the Wallin set made by Stoelting were employed in the induction of after-images. Every *S* before being hypnotized

was shown card 3b which contains a blue disk. The yellow after-image is projected on a white background. All of the *Ss* gave the usual response of yellow when asked what color they saw, when the after-image was projected. The results of one *S* had to be discarded subsequently since he was red-green color-blind. After the *Ss* understood what was required of them, they were hypnotized and the cards 4a and 6c were shown in succession. The card 4a contains a red disk. The after-image green is usually projected on a yellow background which produces a mixture of yellow and green. The *Ss* were told, however, that the original red color was green, so that, if the after-image was influenced by the suggested color rather than the physical stimulus, the resulting color would be red-yellow or orange

TABLE II

THE QUALITY OF THE AFTER-IMAGES NORMALLY EXPECTED, HYPNOTICALLY SUGGESTED, AND ACTUALLY RECEIVED

Stimulus-card	S	Response if physical stimuli were effective	Response if suggested stimulus were effective	Response received
4a	1	yellow-green	yellow-red	greenish-circle
	2	yellow-green	yellow-red	light circle (?)
	3	yellow-green	yellow-red	light yellow circle
	4	yellow-green	yellow-red	pale green circle
	5	yellow-green	yellow-red	light green circle
6c	1	gray	red-blue	white circles
	2	gray	red-blue	light circle
	3	gray	red-blue	gray circle
	4	gray	red-blue	gray circle
	5	gray	red-blue	light green (?)

instead of yellow-green. Card 6c contains a blue disk, the after-image of which is normally projected on blue and produces gray. The hypnotic *Ss* received suggestions to the effect that the original blue was green. The after-image of the suggested color green when combined with blue would produce a red-blue mixture instead of the usual gray.

The results for 5 *Ss* are presented in Table II. It is fairly obvious that the physical stimulus was effective in producing the after-image, since in only two instances were there any doubts. These doubtful responses were caused by the *Ss'* inability to properly designate the color.

These experiments have a direct bearing on three of the theories of hypnosis; namely, dissociation as functional modification, narrowing of the field of consciousness, and heightened suggestibility.

The first of these theories assumes that in some way the normal associative tendencies are broken down and that an event or series of events, usually subject to voluntary recall cannot be reinstated in memory except by some special technique or method.

The second theory while postulating some features of the first, involves a restriction of consciousness so that *S* acts only upon the suggestions given and his consciousness is limited to the facts necessary for carrying out such

suggestion. This theory also necessitates the assumption of restricted initiative.

Increased suggestibility means that *S* will act upon stimulation that would not usually produce a response or at least that the response differs from that which usually occurs. In other words, there is reorganization of the thinking of *S*, so that responses which heretofore have been appropriate, under hypnosis would appear inappropriate if judged by normal criteria. Contrary to the other two theories, increased suggestibility must involve complete integration and complete voluntary control of responses so that the response can be carried out as precisely as possible. For example, amnesia and anesthesia even when apparently present may be pseudo-experiences not comparable to these experiences in normal *S*s.

In regard to the first experiment it may be assumed that all individuals have experienced eye movements during linear or rotational movement. Therefore, we might assume a priori that these individuals would make eye movements when they believe they are rotated. Normal associative processes including memory are involved in complying with the instructions under hypnosis. We can also assume that most people have not analyzed critically the reflex type of movement following rotation and since the movements are involuntary, only simulation of the movements occurs. The results support these assumptions. In Experiment 2, the falling reaction is likewise new to the individual. If there were some direct stimulation of the vestibular function through suggestion we might expect the reaction to take place, even though it has not been previously experienced, since it is an almost invariable reflex. In none of the *S*s could the falling reaction be invoked under hypnosis, until after they had actually experienced the reaction. This again indicates rationalization and the use of normal associative functions.

The pupillary response used in Experiment 3 while not usually subject to voluntary control can be conditioned, and one might safely predict that in conditioned *S*s the pupil would definitely expand with a suggested decrease in illumination. This will not usually happen under hypnosis because the neural function necessary for the production of the phenomena has not been properly organized.

From the point of view of the theoretical implications, perhaps the most interesting experiment of the group is the after-image experiment. If a hypnotized *S* knows the principle underlying after-images, the after-image will always be that of the suggested color. For example, if a hypnotized person knows that the after-image of red is green and that the after-image of blue is yellow, he will report that he sees a yellow after-

image if he is shown a red color and told that it is blue. Wallin's after-image cards are of a sufficiently complex nature to prevent the unversed individual from knowing the correct answer immediately. They are useful, therefore, as a means of ascertaining the effect of suggestion and the effect of the physical stimulus. If *S*s under hypnosis almost invariably give the logically correct answer for the suggestion, when they know what the answer should be and give an answer compatible with the physical conditions when they do not know the correct answer, we have to infer that there is a remarkably high degree of integration taking place. At least, we can safely say that hypnotic suggestion does not modify the mechanism involved in color vision.

All of the experiments point toward a high degree of integration during hypnosis, since *S* has to call upon previous memories to provoke the correct response. It is only in those cases where the response is unknown that *S* tends to go wrong. If dissociation is so highly selective as would be demanded for explaining this type of activity it is not useful as an explanatory concept. It would be invalidated because of the time factors involved. At one instant, a fact of memory would be dissociated yet in the succeeding instant it is utilized in making a response. No special technique has been employed here for restoring the fact to memory.

A narrowed field of consciousness induced by the attitudes of the *S* himself and the technique of hypnosis involves auto-suggestion. The restriction of consciousness is supposed to develop to such an extent that *S* will have no conflicting impulses and only the acceptance reaction. This in turn implies that the volition of *S* is reduced to practically zero. Unfortunately, most hypnotic *S*s manifest a high degree of volitional activity in carrying out suggestions. It would be safer to say that volitional activity is restricted to such stimulation as is given by *E*. This statement would then be in accordance with auto-suggestion established in the manner outlined previously.

All of the experiments which have been carried out in this investigation indicate that the reactions are the results of volitional activity. That is, once the reaction is known to *S* he proceeds to act on the suggestion of the hypnotist. There is no special functional modification in so far as these experiments show. Nystagmus is simulated; falling, following suggested rotation occurs, provided *S* has actually been rotated previously; pupillary expansion does not take place except under actual changes in illumination; and color responses are not modified except in the verbal reports of the hypnotized *S* who is familiar with the phenomena of after-images. All of these responses agree with *S*'s ideas as to what *E* expects. They

appear to have all the elements of active reasoning in which the entire past experience is utilized. When *S* does not know what the response should be, he is unable to make it. For example, in the hypnotic falling reaction after actually experiencing falling, the direction of falling is not always appropriate to the suggested direction of rotation. *S* does not know the relation between direction of falling and direction of rotation.

It seems to the writer that the major difference between a reaction under hypnosis and a normal reaction is the difference between the verbalization of what happened and the actual event. This difference can be explained on a basis of heightened suggestibility, and not by a theory of dissociation or restriction of consciousness.

THE ANCHORING OF AN AFFECTIVE SCALE

By WILLIAM A. HUNT, Connecticut College, and
JOHN VOLKMANN, Harvard University

When an *O* is presented with a group of stimuli, and asked to make absolute judgments upon this group, his judgments give evidence that he has formed an absolute series or scale.[1] The position and the width of this scale are determined by the position of the group of stimuli and by the stimulus-range which the group covers. In general, when the group of stimuli is moved up or down upon the stimulus-continuum, the absolute scale moves with it; when the group of stimuli expands or contracts, the scale likewise expands or contracts. There is still variability of judgment, but the general position of the scale is determined. Under these conditions, it seems appropriate to speak of the scale as being *anchored* by the stimuli. But the position of an absolute scale may be determined by agents other than the stimuli; there are other types of anchoring beside stimulus-anchoring. The evidence for this statement comes from an experiment upon judgments of the inclination of lines, visually presented, recently reported in brief by one of the authors.[2] After judging in six absolute categories a group of inclined lines, the *O*s were instructed to select a value of inclination greater than any which they had hitherto seen in the experiment, to hold this inclination clearly in mind, and to let it define their category *one*. The *O*s then judged the same group of stimuli with this selected inclination defining or *anchoring* the category *one*. As a result of this anchoring the scale shifted toward the anchoring value selected, and the end of the scale nearest the anchoring value shifted most. The shift was not complete, since the scale did not actually extend to include the anchoring value.

For some years the writers have been impressed with certain similarities between the phenomena of psychophysics and the phenomena of affectivity. As a first step toward investigating this similarity, we decided to test the possibility of anchoring effects with affective judgments. If a group of *O*s are asked to judge the hedonic tone of a series of colors in terms of an absolute scale of seven steps, and then at a later sitting are told to select the most pleasant color they can think of, and to let the pleasantness of

* Accepted for publication October 7, 1936.

[1] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493.

[2] J. Volkmann, The anchoring of absolute scales, *Psychol. Bull.*, 33, 1936, 742 f.

this color define the category *seven*, their subsequent judgments of the stimulus-series should show definite changes due to the anchoring effects of this color.

Six *O*s were used; three of them had had previous experience judging the affective values of colors, three had not. None of them knew the nature of the problem. The stimuli were a series of 10 standard Milton Bradley colored papers; they were exposed in a 6-in. square aperture in a large gray screen. The *O* sat about 4 ft. from the screen. A detailed description of the apparatus may be found elsewhere.[3] The series of colors appeared 10 times in constant order, with a 3-min. rest period intervening between the fifth and sixth series of presentations. The individual colors were exposed at regular intervals of approximately 10 sec.

Before the first sitting the *O*s read the following instructions.

You will be shown a series of colors. You are to judge the pleasantness and unpleasantness of each color on a scale of seven steps, represented by the numbers *one* to *seven*. Let the higher numbers stand for the pleasant end of the scale, and the lower ones for the unpleasant end. Report your judgment to the experimenter as soon as it is made.

For the second sitting the instructions were as follows.

You will be given a series of colors. You are to judge the pleasantness and unpleasantness of each color on a scale of seven steps, represented by the numbers *one* to *seven*. Think of the most pleasant color you can; then hold the pleasantness of this color in mind throughout the experiment, and let its pleasantness define the step *seven* on your scale. When you report "seven," you are to mean a pleasantness approximately equal to the pleasantness of the color which you are holding in mind. Report your judgment to the experimenter as soon as it is made.

In the case of four of the *O*s 24 hr. intervened between the sittings; in the case of the other two only 4 hr. intervened. The stimuli and the order of presentation were the same in the second sitting as in the first. At the conclusion of the experiment retrospective reports were gathered from the *O*s as to "what they had been doing." The judgments of the first sitting, under the first instruction, yield one set of results; the judgments of the second sitting, under the second instruction, yield a second set to be contrasted with the first. For purposes of determining the amount of shift in judgment, the mean of the 10 judgments for each color in the first set was compared with the mean in the second set.

The results are shown in Table I, where the stimulus-color, the mean of the first set, the mean of the second set, and the difference between the means are reported for each *O*. The *O*s are represented at the left of the table by the first two letters of their names. After C come the colors in the rank order of their means in the first set. After M_1 come the means in the first set for each color; after M_2 the means in the second set. After D comes the difference between the two means; a minus sign indicates that the second mean is lower than the first, a plus sign indicates that the second mean is higher.

[3] W. A. Hunt, The relation of bright and dull pressure to affectivity, this JOURNAL, 43, 1931, 88.

TABLE I

SHOWING FOR EACH *O* THE STIMULUS-COLORS, THE MEAN (M_1) OF THE JUDGMENTS OBTAINED WITHOUT ANCHORING, THE MEAN (M_2) OF THE JUDGMENTS OBTAINED WITH ANCHORING, AND THE DIFFERENCE (D) BETWEEN THESE MEANS

	C	C	H	J	F	B	I	D	A	E	G
Hu	M_1	7.0	6.0	5.0	4.9	4.8	4.8	4.7	4.0	2.0	2.0
	M_2	6.0	5.0	4.3	3.9	3.0	7.0	5.0	4.0	1.0	2.7
	D	−1.0	−1.0	−0.7	−1.0	−1.8	+2.2*	+0.3	0	−1.0	+0.7
	C	A	F	H	C	I	E	B	J	D	G
Mc	M_1	6.4	5.5	5.0	4.4	4.0	3.4*	3.3	2.6	2.0	1.2
	M_2	5.9	5.7	4.3	4.7	3.1	5.2	3.1	2.5	1.9	1.1
	D	−0.5	+0.2	−0.7	+0.3	−0.9	+1.8*	−0.2	−0.1	−0.1	−0.1
	C	A	F	H	C	I	J	B	D	G	E
Pa	M_1	7.0	6.7	6.6	5.1	4.6	4.2	3.0	1.2	1.1	1.0
	M_2	6.0	5.7	5.9	3.5	4.0	3.0	1.9	1.0	1.0	1.0
	D	−1.0	−1.0	−0.7	−1.6	−0.6	−1.2	−1.1	−0.2	−0.1	0
	C	C	B	A	D	J	H	E	F	G	I
Fl	M_1	7.0	7.0	7.0	6.0	5.0	3.0	1.0	1.0	1.0	1.0
	M_2	4.1	5.0	6.0	3.1	3.6	2.0	1.0	1.0	1.0	1.0
	D	−2.9	−2.0	−1.0	−2.9	−1.4	−1.0	0	0	0	0
	C	J	C	F	H	I	B	A	D	E	G
Ra	M_1	7.0	7.0	6.0	5.6	5.3	5.0	5.0	4.6	1.2	1.0
	M_2	6.0	7.0	4.6	5.0	4.0	4.0	4.1	4.1	1.0	1.0
	D	−1.0	0	−1.4	−0.6	−1.3	−1.0	−0.9	−0.5	−0.2	0
	C	A	F	I	H	C	B	J	D	G	E
Ki	M_1	5.9	5.9	5.1	5.0	5.0	4.2	3.7	3.0	2.8	2.0
	M_2	6.0	5.9	5.4	5.0	5.9	4.0	4.0	2.6	2.9	2.0
	D	+0.1	0	+0.3	0	+0.9	−0.2	+0.3	−0.4	+0.1	0

If an anchoring effect occurs, the scale will shift upward toward the anchoring value. Since the stimuli remain unchanged, they will fall in *lower* steps of the scale after the upward shift has taken place; the scale will tend to move past the stimuli. Hence the stimuli will evoke more judgments in the lower steps of the scale; the means of the second set will be less than the means of the first set, and the differences in the D row will be negative. But the anchoring effect is not a shift of the scale as a whole; it is apparently an *extension* of the scale toward the anchoring value. Both the center of the scale and the length of the scale may be expected to change; roughly speaking, the bottom end of the scale will be determined by the lowest (most unpleasant) stimulus, and the top end by some value between the highest (most pleasant) stimulus and the anchoring value. Since in this process of anchoring the scale is regarded as being fixed at the bottom stimulus and as moving past the top stimulus, the judgments will change more on the upper stimuli than they will on the lower; in terms of Table I, the values in the D rows will be more

frequently negative and (when negative) will have larger negative values, when the mean values M_1 and M_2 are high.

With certain exceptions to be discussed separately, the results in Table I fulfill these expectations. Most of the values in the D rows are negative, a finding which demonstrates a shift of the scale toward a high (pleasant) anchoring value. Further, the values of D are more frequently negative, and negative in larger amounts, when M_1 and M_2 are high, a finding which confirms the view that anchoring involves an *extension* of the scale, and not only a shift of the center of the scale.

The exceptions to these statements do not weaken the case for anchoring. It turned out upon investigation that *Ki* had not understood the second set of instructions. Instead of comparing the affective value of the colors with the affective value of the color he was holding in mind, he was assigning values to the colors on the basis of a comparison of their color with the color he was holding in mind. The basis of judgment was a comparison of colors, and not a comparison of affective values. We are justified, therefore, in considering his results as not pertinent to the experiment. In addition, there are two reversals in the tables which deserve comment. In the case of color *E* and observer *Mc* (item starred in the table), the judgment shifted from 1 to 6 in the middle of the first series and remained at about 6 for the last five presentations. If we accept the mean (5.8) of these last five presentations as the true mean, the presented difference of +1.8 becomes one of —0.6. In the case of color *I* and observer *Hu* (item starred in the table), the mean value of 4.8 on the first series shifted up to 7.0 on the second series because this *O* chose a color very much like color *I* as an affective anchoring value. These are the only two large shifts in the table not in line with our case, but, whether or not these explanations are accepted, the data are still preponderantly in favor of an anchoring effect.

Several features of the experiment go beyond the simple demonstration of anchoring. The reports gathered at the close of the experiment showed rather surprisingly that the *O*s were not in general conscious of the amount and the scope of shift in their judgments. Thus *Ra* who showed a shift for 8 of the 10 colors, said she thought she might have shifted her judgments on "one or two colors," but did not expect to find that she had shifted judgment on 8 of them. Apparently the process of anchoring can be largely unconscious like so many of the processes of judgment. Furthermore, it is to be expected that the amount of shift in judgments due to anchoring will be some direct function of the difference between the anchoring value and the value of the stimuli judged. The statements of the *O*s furnish some slight support for this hypothesis. *Fl* showed the largest shift; she chose as an anchoring color a bright pink, her favorite color; the stimulus-colors she found garish and presumably much less pleasant. *Mc*, who showed the smallest shift (*Ki* excepted), chose a color so close

to the stimulus-series in pleasantness that one stimulus-color was for a short time actually more pleasant than the anchoring color.

Since the experiment did not determine accurately the affective values of the anchoring colors, it is not possible to show that the shift of the scale due to anchoring was incomplete (that the scale shifted toward the anchoring value but did not reach it). While this was true in the original experiment of Volkmann, and may still be true in our experiment, the method was not adequate to demonstrate it.

The experiments of Beebe-Center may be interpreted as showing that the affective scale is determined, within limits, by the position of the group of stimuli which is being judged at the time (as well as by certain other stimuli judged in the past).[4] This finding is called *affective equilibrium* or *hedonic contrast;* however, it may be preferable to regard it as a phenomenon of judgment rather than as a phenomenon of the affective process itself.[5] His finding thus corresponds closely to the condition which we have called *anchoring by the stimuli.* To this form of anchoring there can now be added anchoring by values selected by the *O* and not produced by current stimulation. With the use of anchoring agents the affective scale may increase considerably in extent; we may think of it as changing in the direction of an ideal affective scale, *i.e.* a scale which would include all possible affective values and which would be independent of any particular stimuli presented for judgment. It is very doubtful whether by the use of anchoring the ideal scale can be even approximated. Nevertheless, in the process of making the affective scale more nearly ideal in its independence, affective anchoring and affective equilibrium are important complementary principles.

In conclusion we may state that the anchoring effect demonstrated by Volkmann with judgments of visual inclination may be duplicated with judgments of affectivity. This result suggests that the similarity between certain phenomena in psychophysics and certain phenomena in affectivity is due to the operation of general principles of judgment in both fields. Indeed, many phenomena in the field of affectivity can be explained by invoking the principles of judgment rather than the principles of a unique affective process.

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[4] J. G. Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 54-69.

[5] Beebe-Center, Pleasantness and unpleasantness, in *Psychology,* ed. Boring, Langfeld, and Weld, 1935, 384.

THE RELATIVITY OF ABSOLUTE JUDGMENTS

By NATHAN E. COHEN, Harvard University

Both early and recent experiments employing the method of single stimuli on various psychological variables indicate that there is a large variety of phenomena which involve a common psychological principle: the relativity of judgment.[1] Thus Beebe-Center, after pointing out that the law of contrast implies the "relativity of so-called 'absolute' hedonic judgments," stated: "Before leaving the topic of hedonic contrast I should like to point out that laws formally identical with that of hedonic contrast are applicable to many other psychological variables. Wever and Zener, for instance, have shown that, when observers are made to judge a set of weights in terms of the categories 'heavy,' 'medium,' and 'light,' they soon distribute the categories fairly evenly over the scale, calling the heavier weights 'heavy' and the lighter weights 'light.' Truman and Wever have shown that, with a shift of the range of stimulation, there occurs a shift in the application of the categories 'high,' 'medium,' and 'low' to sets of auditory stimuli. Volkmann, using auditory intensities and lengths of lines as stimulus-variables, has shown that judgments in terms of various sets of absolute categories distribute fairly evenly over the range of stimuli which happens to be in use. These findings have been interpreted by the experimenters as indicating the dependence of absolute judgments on what Wever and Zener have called an 'absolute series,' *i.e.* 'the mental formation which represents the *O*'s conception of the stimuli as a series—the *O*'s organized body of knowledge about the arrangement of stimulus-magnitudes.' "[2]

Wever and Zener in their experiment on the absolute judgment found, furthermore, that in some cases there resulted a skewed frequency-distribution curve with the preponderance of judgments at the upper end of the scale.[3] This result suggested a phenomenon akin to the usual 'negative' time-error in the relative judgment. Pratt, in a more recent experiment, obtained results which tend to confirm this idea, and which led him to the thought that there is probably a common factor involved in both cases.[4] He, like Wever and Zener, explained the absolute time-error in terms of a level effect: "To a measurable extent the judgments are also relative to an intermediate level or standard of reference established by traces of past stimuli which form a homogeneous group with the present stimuli."[5] Needham, proceeding along the lines suggested by Pratt, obtained further evidence of a common factor: he found that the relative time-error also involves a level effect.[6]

* Accepted for publication August 15, 1935.
[1] The writer wishes to acknowledge his indebtedness to Dr. J. G. Beebe-Center for guidance and assistance in this study.
[2] J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness,* 1932, 234-235.
[3] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.,* 35, 1928, 466-493.
[4] C. C. Pratt, Time-errors in the method of single stimuli, *J. Exper. Psychol.,* 16, 1933, 798-814.
[5] Pratt, *op. cit.,* 814.
[6] J. G. Needham, *An Experimental Investigation of the Time-Error,* Thesis, Harvard University, 1933, 168.

Thus, the time-error can be regarded as another among the large variety of phenomena which come under the general principle of the relativity of judgment.

The main trend of explanation through all of these experiments has been in terms of a *level-effect*. Postulation of a level-effect, however, as already pointed out by Beebe-Center in the case of hedonic contrast, is not a complete explanation.[7] If one hopes to obtain a better understanding of the relativity of judgment, it is necessary to go beyond the 'level' explanation; it is necessary to learn more about the details of the phenomenon. The four experiments described below employ a technique which yields a more accurate determination of these details than has been obtained in the past.

Experiments I and II involve unpleasant determiners, that is to say, unpleasant odors presented in order to shift the *O*'s judgments. Experiments III and IV involve pleasant determiners. Since our point of interest lies in the nature of the distribution, it will suffice to present the data of Experiments I and II only.[8] The only difference in procedure between these two experiments was that in Experiment II, in order to show whether the difficulty usually encountered in the use of scales involving gradations of pleasantness and unpleasantness could be avoided, a number scale was used in place of the verbal one. The results turned out to be entirely comparable with Experiment I.

The stimuli were 21 odorous substances, mostly essential oils.[9] Each experiment consisted of two parts. In Part 1 a series of judgments was obtained for 15 of the stimuli. In Part 2, which took place three weeks after the completion of Part 1, 6 unpleasant determiners were added and a series of judgments obtained for the entire group of 21 odors.[10] The method of presentation was such that every odor once followed each of the other odors in the set. This gave for Part 1 (15 stimuli) 210 presentations, divided into 3 experimental hours of 70 judgments each; and for Part 2 (21 stimuli) 420 presentations, divided over 5 experimental hours of 84 judgments each. Adequate rest periods were given during each experimental hour. The instructions called for absolute hedonic judgments in terms of a scale of seven steps extending from very pleasant to very unpleasant.[11]

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[7] Beebe-Center, The law of affective equilibrium, this JOURNAL, 41, 1929, 67.

[8] The data with unpleasant determiners are more univocal than those with pleasant ones, albeit the latter agree with the former. This difference is doubtless due to the difficulty of selecting odors which will be extremely pleasing. The coefficient of correlation between the ranks for different *O*s of the 10 odors having the highest (most pleasant) average hedonic tone is —0.36; that for the 10 having the lowest is 0.46.

[9]
1) Sweet Orange
2) Carnation
3) Jasmin
4) Lemon
5) Bergamot
6) Cinnamon
7) Cloves
8) Cassie
9) Cananga
10) Anise
11) Petit Grain
12) Nutmeg
13) Thyme
14) Fennel
15) Rosemary
16) Juniper
17) Rose Geranium
18) Patchouly
19) Tinct. of Valeric
20) Cod Liver Oil
21) Asafetida

The general technique was the same as that of Beebe-Center, this JOURNAL, 41, 1929, 54-69.

[10] Throughout the experiment the 15 odors used in both Parts 1 and 2 will be called the *test series* and the odors added in Part 2 the *determining series*.

[11] The instructions for Experiment I were as follows: "In the column below you are given seven degrees of feeling. Examine this scale carefully. Become familiar with every item.

There are two ways of treating the results: (a) in terms of the distribution of judgments for all stimuli over the seven categories, and (b) in terms of the relation of hedonic tone to rank of stimuli.

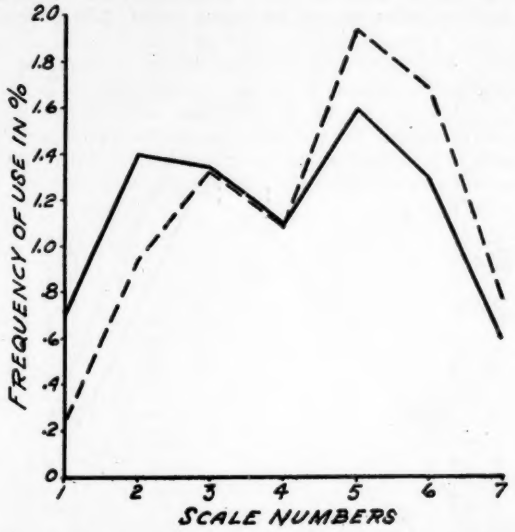

FIG. 1. FREQUENCY DISTRIBUTIONS OF THE JUDGMENTS IN A TEST SERIES BEFORE (CONTINUOUS LINE) AND AFTER (BROKEN LINE) THE DETERMINERS WERE ADDED. FOR ALL Os IN EXPERIMENTS I AND II.

Distribution of judgments. The frequency distribution of the judgments was determined for the test series in Parts 1 and 2. Since there was a difference in

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"Very pleasant
Pleasant
Moderately pleasant
Indifferent, neither pleasant nor unpleasant
Moderately unpleasant
Unpleasant
Very unpleasant

When you are thoroughly familiar with the above scale, a series of odors will be presented. A tapper will inform you that the odor is being presented. Sniff twice. Judge in terms of the above scale the phrase which best describes the degree and quality of your feeling in smelling the odor. State this phrase to the experimenter."

The instructions for Experiment II were as follows: "I am going to present a group of odors for which I want you to give me your affective judgments in accordance with a seven-point scale running from 1 to 7. 1 stands for the maximum amount of displeasure one can have, and 7 for the maximum of pleasure. A tapper will inform you that the odor is being presented. Sniff twice. Judge in terms of the above scale the number that best describes the degree and quality of your feeling in smelling the odor. State this number to the experimenter."

the number of presentations in Parts 1 and 2 the results were made comparable by converting them into percentages. Fig. 1 contains the total frequency curve for all *Os* in Experiments I and II, both before and after the determiners were added.

Inspection shows that following the insertion of determiners there occurred a redistribution of the judgments, with the greatest change at the extreme where the

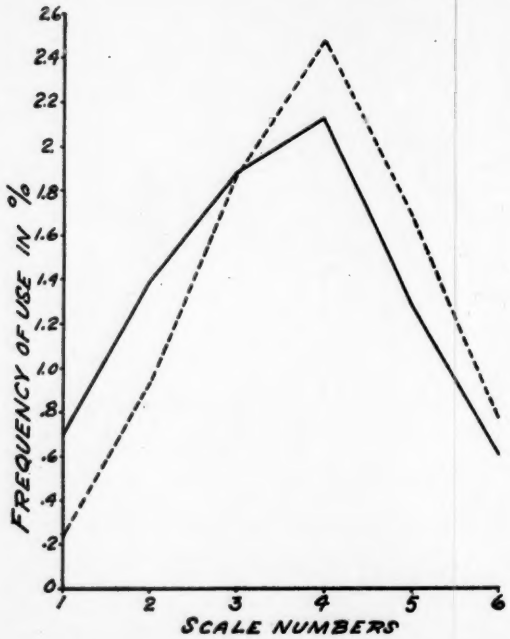

FIG. 2. FREQUENCY DISTRIBUTIONS OF FIG. 1 WITH THE JUDGMENTS OF THE INDIFFERENT CATEGORY DIVIDED EQUALLY BETWEEN THE —1 AND THE +1 CATEGORIES

determiners were added. For example, the change was —0.46 at —3, but only +0.17 at +3; —0.46 at —2, but only +0.39 at +2; and —0.01 at —1, but only +0.35 at +1.[12] The changes around the region of the indifferent point were artifacts of this category. (This problem will be dealt with in a later paper.) If, for the present, the indifferent judgments are divided equally between the —1 and +1 categories, a clearer picture, as shown by Fig. 2, is obtained.

The results are consistent with the type of redistribution which would occur

[12] A *minus* sign indicates a decrease in the number of judgments, a *plus* sign an increase.

in the case of a normal distribution anchored at one end and placed in a circumstance which causes shrinking of the seven-category scale to six categories. The shrinkage in this case would not be uniform all along the scale but rather one that must be expressed by the term kx; k being a constant value, representing the proportion of the original curve to which the complete curve has shrunk, and x the distance of the measured point from the stationary end. The value to which any point along the scale has been reduced would then be given by kx. Theoretically, however, the natural assumption would not be shrinkage of the scale but rather redistribution over the entire seven categories, with the judgments on the inserted determiners usurping the extreme ones necessary for their number. Fig. 3 which represents the frequency distribution for all of the judgments of Part 2, indicates that there was some such redistribution involved.[13] The effect in this case also

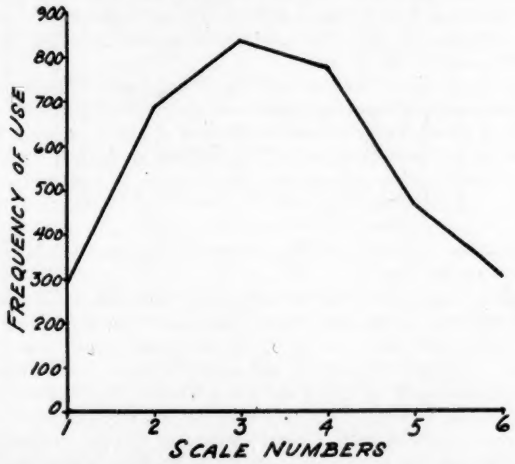

FIG. 3. FREQUENCY DISTRIBUTION OF THE JUDGMENTS ON THE TEST AND DETERMINING SERIES IN PART 2. FOR ALL Os IN BOTH EXPERIMENTS I AND II

would not be uniform all along the scale, but would decrease with remoteness from the affected extreme.

Due to a lack of time it was impossible to make a final check as to which of the two assumptions is correct. A treatment of some of the individual cases, however, indicated that the second assumption (redistribution over the entire scale) is probably correct. The proper approach would be to use a small number of stimuli so that the rank-order will remain constant throughout the experiment, for,

[13] Since the indifferent category, as has already been suggested, does not appear to be a category coördinate with pleasantness and unpleasantness but merely an ideal limit between them, its judgments were divided equally between the —1 and +1 categories.

even though a specific redistribution may be the principle involved, the average set of results, due to variability, will yield a distribution which will be a compromise between the original and the ideal curve.

Relation of hedonic tone to the rank of stimuli. Since contrast can be described in terms of a redistribution of judgments over the categories, the consequence with respect to individual stimuli in a hedonic series should be that the effect of determiners at one end would decrease with the remoteness of stimuli from that end. That such a result is indeed the case can be seen by inspection of the curves of Fig. 4, which were derived in the following manner. In both experiments the judgments were converted into their plus and minus equivalents. In the case of every *O* an average value was determined for each odor in Part 1. The 15 average values were then arranged in an ascending order and an average value determined for all four *O*s. These results were plotted in a graph, of which the abscissa represented the ascending series of odors, and the ordinate the numerical scale running from —3 through 0 to +3. The same treatment was given the judgments of Part 2 which involved 21 odors.

On the basis of these curves it ought to be possible to formulate the law of contrast in mathematical terms. An inspection of the curves indicates that an accurate formulation of the relation of hedonic tone to rank of stimuli probably is a sigmoid function. They also show, however, that a straight line might constitute a fairly good approximation. This approximation can doubtless be attributed to variability which, according to Thorndike, would be greater at the extremes than at the center; for, with the greatest variability at the extremes, the curve would tend to become 'snake-like' in shape and hence, on the average, would yield results which are close to a straight line.[24]

After further experimentation the mathematical law will probably be expressed in terms of the more accurate formulation; namely, the sigmoid function. At present, however, the straight line, $y = ax + b$, is a good enough approximation to warrant its use. The b constant in the equation would be determined by the range of judgments (*i.e.* the scale) and the a constant by both the range and the number of stimuli involved. In the case where all categories were used, where hedonic value is defined with 0 corresponding to extreme unpleasantness, and where the first stimulus is given the rank of 0, the formula defining the hedonic value h of a stimulus of a given rank r would be $h = rN/n$ in which N equals the number of steps in the scale (one less than the number of categories) and n equals the number of intervals in the stimulus series (one less than the number of stimuli). For this particular case, if contrast effect is indicated by Δh, the law of contrast would be $\Delta h = r_2(N/n_2)r_1(N/n_1)$ in which the subscripts 1 and 2 indicate the order of the distribution, 1 being before the determiners were added, and 2 after.

Discussion

The treatment of the results in terms of the distribution of judgments is systematically important because it deals with the general problem of the relativity of judgment. The results show that, given a series of stimuli, the judgments of them will tend to distribute themselves over the categories in a manner which is

[24] E. L. Thorndike, The influence of chance imperfections on the relation of initial score to gain or loss, *J. Exper. Psychol.*, 7, 1924, 225.

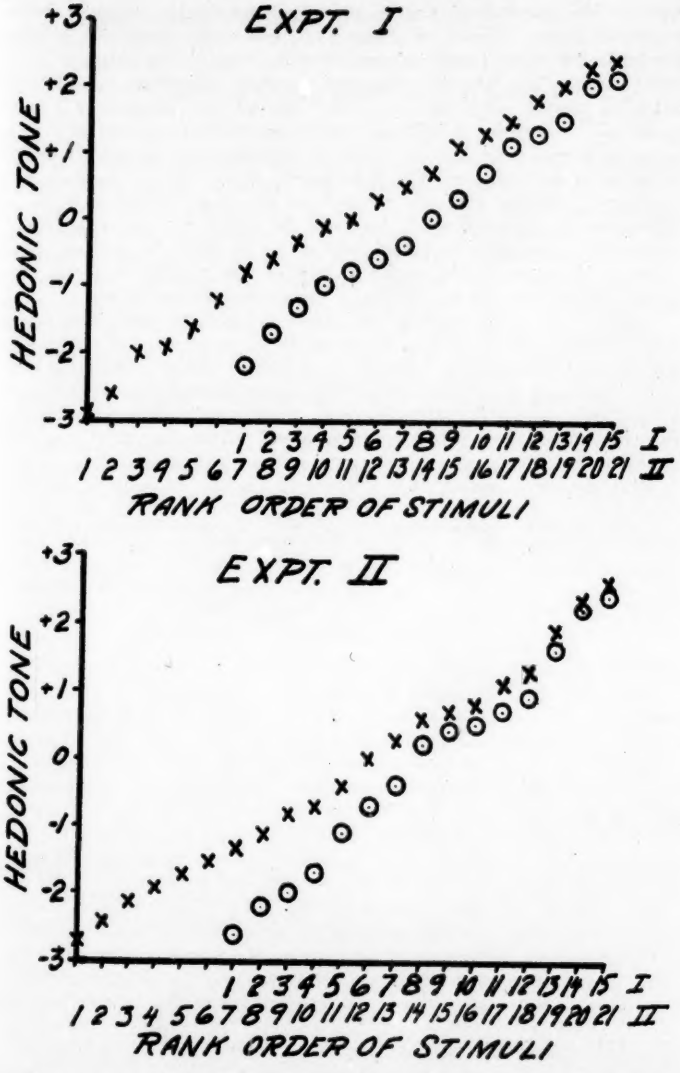

FIG. 4. THE CIRCLES (SCALE I) SHOW THE HEDONIC RANK-ORDER OF THE TEST SERIES (PART 1) FOR ALL 4 OS. THE CROSSES (SCALE II) ARE FOR THE TEST AND DETERMINING SERIES (PART 2)

typical of the normal distribution. If more stimuli are added to the series, furthermore, there appears to result the formation of a new normal distribution, involving a redistribution of the former judgments to make room for the judgments of the added stimuli. Thus, what has been termed hedonic contrast and related to the immediate effect of *stimuli* or traces of these effects ("level phenomenon") proves to be rather a relativity of judgment based upon specific frequencies of a set of *responses*. Knowing the scale prescribed in an experiment but not the stimuli, prediction of response is possible. Knowing the stimuli but not the scale, there can be no prediction. Expressed paradoxically this is to say that it is not the pleasantness of a stimulus which determines the judgmental response but the response which determines the pleasantness of the stimulus. As we have already noted in the section on the relation of hedonic tone to rank of stimuli, these judgmental responses can be described in terms of a mathematical law.

Needham, in a recent experiment on contrast with auditory intensities as stimuli, secured results which parallel closely the results of the present experiment: "The contrast effect which operates when a series of stimuli (auditory intensities) is preceded immediately by another series of significantly different intensity is most (if not entirely) effective in that region of the stimulus-scale which is most remote from the intensive level of the preceding series."[15] Hence the relativity of judgment described above applies not only to pleasantness and unpleasantness, but also to other absolute judgments. It is a feature of absolute judgments in general.

[15] J. G. Needham, Contrast effects in judgments of auditory intensities, *J. Exper. Psychol.*, 18, 1935, 222.

GROUPING AND THE SPAN OF APPREHENSION

By GERTRUDE RAFFEL, Monmouth Junior College

In 1885 Cattell studied the effect of grouping on tachistoscopically exposed material.[1] He compared with each other disconnected lines, lines combined in the form of letters, letters combined as words, and words combined as sentences; and he found an increasing accuracy in report as the perceived material was thus arranged in larger groups. Boring has suggested two possible interpretations of this result.[2] (1) Grouping may increase the range of correct apprehension; more details may actually be 'perceived' in the sense that more can be correctly reported afterward. (2) On the other hand, it may be that the capacity of the organism to respond discriminatively to a range of material is not actually increased by grouping of the material. In this case, increased accuracy of report becomes an artifact of surrogation. A more organized material may be represented by fewer terms than a less organized material, because a single term may stand for a part-whole. Such part-wholes are more inclusive and thus fewer when organization is greater. This second alternative would shift the problem of any increase in the span of apprehension as the result of increased organization to the question of the basis of surrogation, presumably in the past experience of the organism. The present experiment was undertaken with the object of deciding between these alternatives by the use of material which is meaningfully organized but which can not be correctly reported by means of surrogation.

PROCEDURE

The material consisted of 14 pairs of cards (cf. Fig. 1). On one member of each pair was a diagrammatic representation of a face; the same features were drawn in haphazard grouping on the other card, arranged to form a nonsense figure of the same size as the corresponding face. Each face consisted of 8 details. No two mouths, eyes, noses, ears or facial outlines were the same shape. In scoring the reproductions, one point was given if the shape of a detail was drawn correctly. Size and position were disregarded. Under these conditions the advantage of guessing is minimized: if the *O* remembered only that there was a nose on a picture, there was little likelihood of his drawing by chance the nose which had just been shown him. The material thus permits a comparison between reproductions of meaningful and of nonsense figures, in which the presence of surrogates does not increase the accuracy of the reproduction of the meaningful figures. If the scores of the faces should be found to be higher than the scores of the nonsense figures, then it would appear that meaningful organization increases the range of apprehension in respect of the number of details that can be correctly per-

* Accepted for publication June 15, 1935. This study was made at Radcliffe College.

[1] J. McK. Cattell, Über die Trägheit der Netzhaut und des Sehcentrums, *Philos. Stud.*, 3, 1885, 94-127, esp. 121-127.

[2] E. G. Boring, *The Physical Dimensions of Consciousness*, 1933, 194-198.

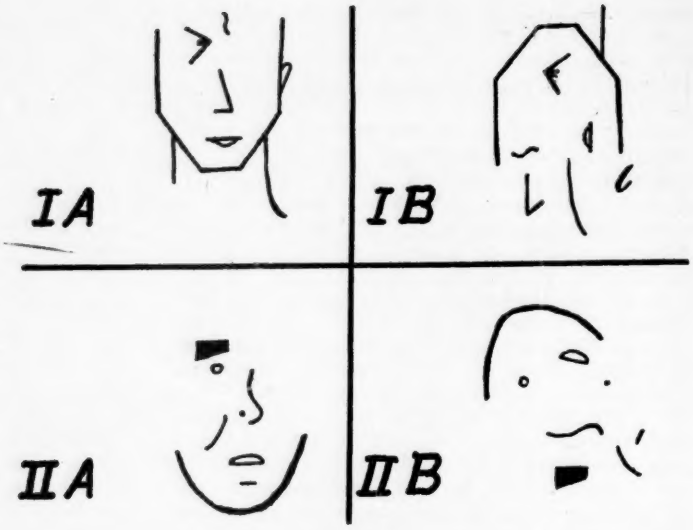

FIG. 1. EXAMPLES OF MATERIALS

IA and IIA are the faces. IB and IIB are respectively the corresponding nonsense figures, made up in each case of the same elements as the faces. There were 14 such pairs of stimuli.

ceived. On the other hand, if the two scores are not significantly different, it would seem probable that the advantage of meaningful organization in Cattell's experiment was due to surrogation, that the *O* reacted as if the details were functioning when actually they were not.

Four methods of presentation were used. Either each card was shown once, with tachistoscopic exposure time of 60 millisec., 2 sec. or 10 sec.; or each card was shown repeatedly for 2 sec. until the *O* could reproduce all of the details correctly. Under the latter procedure, no card was shown more than 10 times. With tachistoscopic exposure the *O* was given enough practice trials to familiarize him with the procedure. Under the other conditions he was given one practice trial.

The *O* reproduced by drawing. Each drawing was made on a small slip of paper and immediately given to the *E*, who examined it to see if there would be difficulty in scoring. In case of such difficulty, as when a line might be U-shaped or circular, she asked the *O* what he had intended to draw. Except for these instances, the *O* did not see his drawings after he had made them. Interference between the figures was never reported and was not observed by the *E*.

Besides a description of the procedure, these instructions were given the *O*. "You will be shown a nonsense figure. As soon as it has been exposed, draw it as accurately as you can. Draw only the details you are sure of; don't try to give the general impression. For instance, if you know there was a line in one corner, but don't know

whether it was straight or curved, don't draw it at all." "You will be shown a drawing of a face. After it has been exposed, draw it as accurately as you can. Don't expect to see a real face; it will look more like a caricature or a comic strip, with conventionalized features. And don't think, just because it's a face, that it's sure to have two eyes and two ears and a nose and a mouth. Draw only what you're sure of; don't try to give the general impression. The scoring is on the basis of the number of details you get right, not on the general effect."

All the faces and all the nonsense figures were shown in series. With each method the faces were shown before the nonsense figures for half the Os, and after them for the other half; thus any practice, transfer or fatigue effects which may have been present were offset in the averages.

TABLE I

AVERAGE SCORES WITH SINGLE EXPOSURE OF THE MATERIAL

(Perfect score = 8. Significance of difference varies from 0.500 = no significance, to 1.000 = completely significant.)

	60 ms. exposure 96 reproductions (8 *Os*)	2 sec. exposure 100 reproductions (10 *Os*)	10 sec. exposure 104 reproductions (8 *Os*)
Faces	1.6	3.2	5.5
Nonsense figures	1.6	3.3	4.7
Significance of difference	.500	.614	.999

TABLE II

AVERAGE SCORES WITH REPEATED EXPOSURES OF THE MATERIAL

(Each score in the table is the mean of 40 reproductions: 4 drawings were shown to 10 *Os*. Perfect score = 8.)

Trials	1	2	3	4	5	6	7	8	9	10
Faces	3.0	4.7	6.3	6.9	7.6	7.6	7.8	7.9	7.9	7.95
Nonsense figures	3.0	4.3	5.4	6.2	7.0	7.6	7.8	7.9	7.9	7.9

RESULTS

Table I compares the average scores on faces and nonsense figures after a single exposure of the material. There is no difference between them with either a 60-millisec. or a 2-sec. exposure; but the faces are reproduced more accurately than the nonsense figures when the exposure time is 10 sec. This difference is statistically significant; according to "Student's" formula[3] P = 0.999

Table II corroborates Table I. After the first 2-sec. exposure there was no difference in score between the two types of material. After the second exposure the faces were drawn more accurately, and this tendency became more marked after the third exposure. These scores are comparable to the scores after a single 10-sec. exposure, reported in Table I, since in both cases the material was a little more than half learned. In both cases, also, the faces were reproduced more accurately than the nonsense figures. The data should be interpreted in terms of the information provided by the introspections.

Almost all of the *Os* reported that the faces were more organized (or unitary or connected) than the other figures, and that the faces seemed to have fewer details. With short time intervals, the *Os* reproduced the same number of details for both

[3] "Student," The probable error of the mean, *Biometrika*, 6, 1909, 1-25.

kinds of material; one would therefore expect them to consider the nonsense figures more difficult. In general, however, they were dissatisfied with their reproductions of the faces, because they could recall so many impressions which they could not reproduce. One *O*, for example, having completed her drawing, said, "There was a scowl, but I don't know how to draw it." In the original (Fig. 1, IA), the scowl was represented by a wavy vertical line in the middle of the forehead; the detail had been forgotten, although its surrogate was recalled. With short times of exposure, therefore, both a general impression and a certain number of details can be perceived; but the two are independent of each other to a surprising degree.

With a longer time of presentation, the *O*s reported that during exposure of the faces they tried to find relations between the features, or to associate one of the features with another more familiar way of representing it. They would think, for example, "Both the nose and the mouth are curved," or "The nose is a triangle with one side left out, like a child's drawing." Under these conditions the faces were reproduced more accurately than the nonsense figures. It thus appears that, when the exposure is long enough to permit an active search for relationships, meaningful organization of the details that are presented is an aid in the reproduction of those details.

Summary and Conclusions

In order to discover whether the grouping of elements increases the span of apprehension, faces composed of conventionalized features and the same features in random arrangement were presented. The *O*s were instructed to reproduce details and not general impressions. The material was scored according to the number of details correct. The *O*s reported that the faces seemed to be more organized than the nonsense figures and to be composed of fewer details; therefore the material affords a valid test of the effect of grouping.

When the time of exposure was short, the same number of details was reproduced for both kinds of material. The *O*s were often able to report certain aspects of the faces, such as the sex or expression portrayed, but unable to recall the details which gave rise to the impression. These facts indicate that grouping of units increases the span of apprehension only if the units used can be inferred from surrogates.

With longer exposures, more details could be reproduced for the faces than for the nonsense figures, and the faces could be learned more readily. The introspections show that, with longer times of presentation, the *O*s analyzed relationships among the grouped details or formed associations with them. When the conditions of presentation permit such analysis, meaningful organization of elements aids their recall.

FURTHER STUDIES OF BODILY SWAY

By E. B. Skaggs, Wayne University

The following paper reports the third of a series of studies of bodily sway.[1] In this study we have attempted to obtain information concerning several experimental conditions which we did not investigate in the earlier ones.

Apparatus. Since the apparatus has already been described,[2] brief mention of it here will suffice. There are three integral parts to it: a body-piece, a meter stick with a cardboard platform attached to the upper end, which is fastened to *S*s back—the platform projects about 2 in. above his head; a recording sheet, smoked paper, which is fastened to the top of the platform; a stylus, an adjustable overhead rod that moves freely in the vertical direction in a sleeve that prohibits horizontal play, which is brought to bear upon and removed from the smoked paper as desired.

General procedure. S stood under the overhead piece with his feet in a constant standard position, *i.e.* with heels 2 in. apart and feet turning outward at an angle of 20°. *S* was instructed to assume a comfortable, erect position and to signal by saying "Now" when he found one which he thought he could maintain.

Obviously it is very important that *S* should first attain a well-balanced, stable position of the body before giving the signal "Now." Also, it is necessary that *S* understand that if he feels himself moving away from this initial position he must strive to get back to it. Consequently we took especial pains to see that every *S* understood all this clearly. Every *S* understood that we measured only the maximum distance that he traveled away from this starting point.

When *S* gave the signal, *E* brought the stylus to bear upon the smoked paper and at the end of the desired test-interval, removed it. In every experiment *S* stood with eyes open and with attention directed to the task of maintaining the initial position, attending specifically to the kinesthetic tensions in the muscles of the legs. The tests were rotated from day to day for every *S* and a counter-balanced order of procedure was used within each group. As in the earlier studies, we have taken the greatest deviation from the initial position as the measurement of bodily sway. All measurements were taken in terms of millimeters.

Experiment I. In this experiment we wished to test both the effects of different degrees of muscular tension and the effects of different lengths of test-intervals on bodily sway.

Subjects. Twenty-five young women, college sophomores, were selected at random from the writer's beginning classes in psychology and served as *S*s in this experiment.

Procedure. Two conditions of muscular tension were used. In one case *S* was

* Accepted for publication April 10, 1935. This study was made with the assistance of Ivah S. Skaggs.

[1] For the earlier studies see E. B. Skaggs and G. C. Eichkern, Some studies of body sway, *Mich. Acad. Sci., Arts, & Letters,* 10, 1928, 369-379; E. B. Skaggs, I. S. Skaggs, and M. Jardon, Attention and Bodily Sway, this Journal, 44, 1932, 749-755.

[2] Skaggs and Eichkern, *op. cit.,* 369-371.

asked to stand with just normal tension of the muscles of the legs. In the other case, *S* was asked to tense the muscles of the legs to a greater degree than normal.

Since it was very necessary that our *Ss* should all understand the meaning of *normal* and *super-normal* tension we spent some time in rehearsing these conditions and defining them. We asked every *S* first to stand comfortably erect and with muscles tensed in usual manner. This was defined as a normal condition. Then *S* was asked to tense the muscles until they were almost painfully rigid. This position was then pointed out as one that we did not desire, as it was extreme. *S* was then asked to go back to a normal condition of tension and then tighten up the muscles half-way between this condition and the extreme condition. In spite of our attempt to standardize these two conditions by means of explanation and rehearsal it is probable that some *Ss* were more tense in the super-normal condition than others and that some *Ss* were more relaxed in the normal condition than others.

Four different lengths of test-interval were used under each of the above two conditions; namely, a 15-, 30-, 45-, and 60-sec. interval. Eight records (a complete cycle of tests) were obtained at every meeting. At the first meeting every *S* was given the proper instructions and had a preliminary practice on every one of the tests. Thereafter, every *S* came to the laboratory 5 times, thus affording 5 records for every condition investigated. At every meeting, between the first and last four tests, *S* was relieved of the apparatus and sat relaxed for 4 min. All experiments were performed in stocking feet.

Table I presents the group averages and variations. The group averages are based upon the averages of the 5 records for every one of the 25 *Ss*.

The data of Table I show that, in all cases and regardless of length of test-interval used, there is greater deviation from the starting position when *Ss'* muscles are tensed to a super-normal degree. Evidently there is a critical degree of muscular tension for maximum control. If the muscles are tensed beyond this point there is a decrease in motor control.

Under the conditions of both normal and super-normal tension the data show that there is an increase in the amount of deviation from the starting position with increase in test-interval. The increase is, however, not proportional. The figures shown in Table I may be summarized and expressed in percentages as follows:

Under normal muscular tension (maximal deviation):

An increase of 100% test-interval gives an increase of 35%
An increase of 150% test-interval gives an increase of 61%
An increase of 200% test-interval gives an increase of 73%

Under super-normal muscular tension (maximal deviation):

An increase of 100% test-interval gives an increase of 33%
An increase of 150% test-interval gives an increase of 51%
An increase of 200% test-interval gives an increase of 85%

Presumably, if we had used longer intervals an interval would have been found where no further increase in sway, as we have measured it, would have been found.

Experiment II. In this experiment we were interested to find out whether there was any difference in amount of sway according to whether *S* stood with shoes on or in stocking feet. Also, we wished to use a third condition of muscular tension, in which not only the muscles of the legs would be tensed above the normal but the muscles of the arms would be likewise super-normally tense.

Subjects. Twenty-five young women, students in the writer's beginning classes in psychology, served as *Ss*.

Procedure. In all cases in this experiment the length of the test-interval was 1 min. All *Ss* stood with eyes open and with attention directed as in Experiment I. After one practice meeting, in which *S* took the complete series of 6 tests, the

TABLE I

AVERAGES FOR THE TWO CONDITIONS OF MUSCULAR TENSION AND FOR DIFFERENT LENGTHS OF TEST-INTERVAL

	Normal tension				Super-normal tension			
	15″	30″	45″	60″	15″	30″	45″	60″
Mean	16.2	21.9	26.1	28.1	17.5	23.3	26.5	32.4
M.V.	3.66	5.53	5.98	6.79	4.68	5.60	5.97	8.20
S.D.	4.3	6.3	8.7	7.7	5.6	5.9	8.0	10.5

TABLE II

AVERAGES FOR THREE CONDITIONS INVESTIGATED WHILE STANDING WITH SHOES ON AND IN STOCKING FEET

	With shoes on			In stocking feet		
	I	II	III	IV	V	VI
Mean	31.1	29.1	27.9	31.3	29.8	28.0
M.V.	7.23	4.94	5.10	6.36	6.41	5.75
S.D.	9.60	7.40	6.80	8.30	9.50	6.70

regular meetings were started. As in Experiment I every *S* came to the laboratory twice a week and was given 6 tests at every meeting. Six regular meetings were arranged with every *S*. The tests were rotated for every *S* and a counterbalanced order of testing was followed.

The data, which are presented in Table II, are ordered under Roman numerals, the meaning of which is as follows.

I. Shoes on—muscles of legs tensed, arm muscles normal.
II. Shoes on—muscles of legs tensed, arm muscles also tensed.
III. Shoes on—muscles of legs normal, arm muscles normal.
IV. Shoes off—muscles of legs tensed, arm muscles normal.
V. Shoes off—muscles of legs tensed, arm muscles also tensed.
VI. Shoes off—muscles of legs normal, arm muscles normal.

Again the data show that, on the average, *Ss* sway more with muscles of the legs tensed above the normal. One notes that, however, when the arm muscles are also tensed along with the super-normal tension of the muscles of the legs, the amount of sway is not so great as when just the muscles of the legs alone are tensed. In other words, the tensing of the arm muscles seems to help the *S* in maintaining the initial position, although it cannot offset the disturbing effects of the super-normally tensed muscles of the legs.

The reader will note that there are practically no differences between the records taken under conditions of shoes on and shoes off. Most of our *Ss* were sure that they could maintain position much better with their shoes on and were later surprised at the results. It should be noted that the women of our group wore heels of medium height, ranging in height from 1½ to 1¾ inch. With very high heels it is possible that our figures would not hold.

CONCLUSIONS

(1) In all cases our *Ss* swayed more with muscles of the legs tensed above the normal.

(2) When the arms are tensed above normal, along with super-normal tension of the muscles of the legs, the disturbing effect of the later condition seems to be partially offset.

(3) Using intervals of 15, 30, 45, and 60 sec., it was found that the *Ss* swayed more with increase in length of test-interval, but not proportionally. The data indicate that if we had used longer and longer intervals, an interval would have been found in which there would be no further increase in amount of sway, according to our method of measuring sway.

APPARATUS

A SELF-CALIBRATING TIME-CONTROL FOR MULTIPLE CIRCUITS

By Harry Helson and Norman Powell, Bryn Mawr College

The necessity for controlling the length of time between make and break and from the breaking of one electrical circuit to the making of another has led to the development of the time-control which we shall describe here. The various features of the instrument appear more clearly if we consider a concrete case in which it has been used, for example, to control the time that a sound is given, followed, after a controlled time-interval, by a flash of light the duration of which is also controlled. To be truly universal an instrument designed for such time control should possess the following features: (1) the circuits must be entirely independent; (2) changes in time must be easy and rapid; (3) calibration of the instrument should be easy and without reference to outside standards; (4) large currents must be taken care of; (5) the instrument should satisfy certain criteria of accuracy; and (6) it should hold its calibration with a reasonable amount of use and under a variety of conditions. We believe the instrument described below to fulfill these requirements better than any timing device with which we have worked or are familiar.

The instrument employs two principles, one entirely new in the history of timing-devices: (1) the control of time by means of a crank and piston motion, the time between make and break being a function of the length of a rod which makes contact with a piston; (2) control of time-interval (*i.e.* time between breaking of one circuit and the making of the next) by displacement of the geared crank disks carrying the pistons in such a way that the riding-rods make contact with the pistons successively. By adjusting the length of the riding-rod the length of time an electrical circuit is made is controlled, and by revolving the crank disks carrying the pistons the time between circuits is controlled.

The instrument is constructed as follows. The driving unit consists of a Bodine synchronous motor (M), 1/20 h.p., 1800 r.p.m., with a 30 : 1 worm gear speed reducer which is mounted on a cold rolled steel bed plate, 5/8 in. thick, 6 in. wide and 18 in. long, the whole set on four legs provided with rubber pads to reduce vibration (see Fig. 1). Two upright, steel posts (7 and 7 A in Fig. 1.), 15.5 in. high and 1 in. square thickness with ends turned to 3/4 in. round and threaded so as to be held to the bed plate by a 3/4 in. hex nut on the under side of the plate, serve as the uprights for the frame which is completed by cross-bars of cold rolled steel fitted into milled slots in the uprights. The frame holds the guides for the connecting rods, riding-rods, switches, sockets, and the various accessories shown in the figure.

The speed reducer shaft, set at right angles to the motor, has a speed of 1 r.p.s. and has mounted on it two 36 pitch gears, one with 25 teeth and the other with 100 teeth. The second geared disk, which is also the crank disk, has a hole 1 in. off center in which is the crank pin holding a piston. On this piston is a

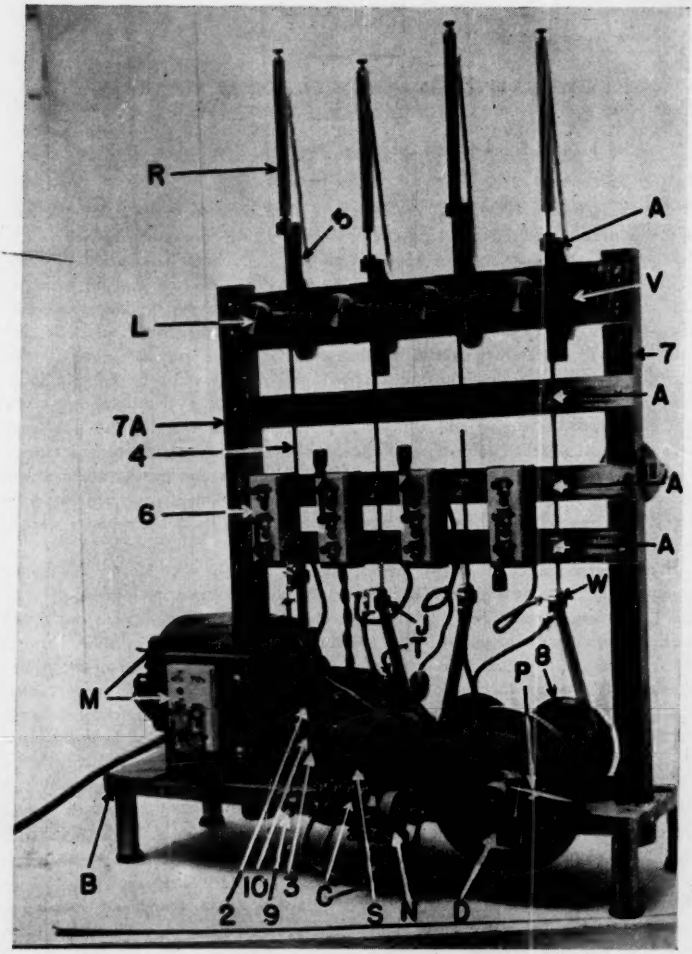

FIG. 1. SELF-CALIBRATING TIME-CONTROL APPARATUS

Bakelite joint (J) held by a ⅛ in. wrist pin which is tapped to hold a ⅛ in. connecting rod (4). This is faced on its end with spark plug metal to insure clean make and break. This rod acts as a piston and is kept in place by two square steel guides (A) fastened to the lower steel cross bars. These guides are bushed with amber so as to insulate each rod from the other through the frame. A piece of

bent brass (W) in the joint (J) taking the place of a lock nut is drilled and tapped for a screw to serve as a binding post. Directly above the connecting rod is another rod, called the riding-rod, (5), set in two more guides, the upper of which is movable over a space of 2 in. The upper guide moves in a slot (V) marked in Vernier fashion and reading to 1/1000 in. A knurled screw (L) holds this guide at any setting.

The second rod, also faced with spark plug metal, can be moved up or down, thus controlling the length of ride on the piston. To insure solid make and complete contact during the change of motion when the connecting rod turns from its upward to its downward stroke, the riding rod is screwed into a ⅜ in. rod (R) about 6 in. long, and provided with a screw binding post for the wire controlling the circuit. So long as the riding rods and pistons are in contact, the circuit is made and when the riding rod has reached the end of its excursion, the circuit is broken. The use of spark plug material helps to prevent sparking and oxidation at the contact surfaces.

In order to control several stimuli it is necessary to duplicate the above design for as many controls as one wishes. The Bryn Mawr instrument is provided with arrangements for four circuits. The control of the duration of each stimulus is obtained in the manner just described but the control of the time *between* stimuli involves the following design: meshing into the geared disk (3) attached to the shaft of the reducing unit are three more geared disks with 100 teeth in each and driven by the first one. These are provided with crank and connecting rods as described for the first unit and are mounted on ½ in. steel studs driven into steel blocks which are fastened to the bed plate. In order to change the time-interval between stimuli, the geared disk is capable of displacement with respect to its neighbors by sliding over the back disk. Springs supply the tension between the driving and geared disks. The geared disks can be taken out of mesh for displacement with respect to each other. Since there are 100 teeth on each disk, displacement by a single tooth gives a time-interval of 1/100 sec. Displacement of these disks with respect to each other governs the time-interval between circuits. A zero line on each disk when lined up with that of neighboring disks indicates when all four rods are moving in unison. When all connecting rods are moving in unison, one would be making and breaking the circuits simultaneously if the riding-rods are all of the same length. The Vernier adjustment at V allows fine settings of the riding-rods so that times as little as 10 millisec. can be obtained. Time-intervals to within a fraction of a full second are possible with the present arrangement of the apparatus but longer times are possible by changing the design of the star-wheel unit which we shall now describe.

Since in most experimental situations the experimenter does not wish to present stimuli once every second, a modified star-wheel device has been incorporated in the instrument which cuts the timer out for three seconds and brings it in on the fourth. This is accomplished by means of a 100 tooth gear directly under and meshing into the 25 tooth gear on the speed reducer shaft. This reduces the speed from 1 r.p.s. to ¼ r.p.s. The motion is then translated from the 100 tooth gear by means of a shaft to miter gears (9) and then to the cam shaft. Form hardened steel cams (C) are inserted in the cam shaft and these four cams (one for each circuit) open and close four switches (S) which are similar to the breaker points

of an automobile distributor. These switches consist of rocker arms riding on the cams. The adjustment of these rocker arms makes it possible to change the length of time the switches are closed. It is necessary to change the position of the cam on the cam shaft when any geared disk is displaced in order that the rocker arms shall be in unison with their respective riding rods. The cams can be rotated with respect to each other by releasing the knurled nut (N) at the end of the cam shaft.

We now come to the calibrating unit which is built into the timer and appears as a disk (D) with a pointer (P) at the right of Fig. 1. The circuit to be calibrated is tested by inserting a neon lamp which is lighted at the make and extinguished at the break of the current. A knurled knob (T) has been mounted on the motor shaft to enable one to turn it by hand, which is necessary in order to read the dial setting when calibrating. At the moment the test lamp lights, showing the circuit is made, the pointer P is moved to zero position, always in a counter-clockwise direction to take out any backlash in the gears and shafts. The motor shaft is turned by hand until the test light goes off and the position of the pointer at this instant is noted. The calibrating dial is divided into 200 divisions and the pointer is geared to make 5 r.p.s. by means of a shaft and gear with 20 teeth meshing into the last crank disk of 100 teeth. Each division on the dial therefore represents 1 millisec. If, for example, the test light has been on from 0 to 175, the time between make and break is known to be 175 millisec. for that circuit. Sockets have been mounted on the rear side of the frame for testing lights. Similarly the time-intervals between the breaking of one circuit and the making of the next can be measured by taking readings of the pointer position when one test lamp goes off and the next one comes on.

Several comments as to the actual operation of the timer may be of interest. The present instrument is very quiet and can be rendered almost completely noiseless by enclosing it in a properly made box. In this case the temperature of the motor must be guarded in warm weather unless a fan is used to cool it. The Bryn Mawr timer is kept in a small room at some distance from the rooms in which the experiments are being conducted. The timer could be put in any part of the laboratory and made to actuate electrical circuits in different experiments, provided they were in progress at different times. That the instrument holds its calibration is proved by the fact that after as much as 20 hours' use, no detectable differences in time were found. It is obvious that extraneous factors may operate to reduce the accuracy of the timer at extremely small intervals, *e.g.* arcing due to heavy currents, etc. We have used our instrument with currents as large as 5 amp. at 110 v. Breaker points on rocker switches and on the pistons and riding-rods must be kept clean, and sufficient weight must be applied to these to insure firm contact of these parts during the period of make.

The instrument described here is smaller than the original, experimental model employing the same principles of operation. An even smaller apparatus can undoubtedly be built, but ruggedness of construction contributes to stability and accuracy of timing. If only a single electrical circuit is to be controlled, only one unit would be necessary. Often, however, it is necessary to provide for signalling stimuli and other accessories in addition to the actual stimuli employed in experimentation, in which case several electrical circuits must be provided for independently. The actual number of units (pistons, driving rods, etc.) to be built

into any model depends upon the number of electrical circuits which must be independently controlled. The Bryn Mawr models with four units have been found satisfactory for as many as three stimuli and a signal light. The instrument lends itself to many uses and various modifications are possible while still retaining the piston and displaced-disk principles employed here.

A TWIN-OSCILLATOR FOR AUDITORY RESEARCH

By S. S. Stevens and R. Gerbrands, Harvard University

The instrument described here was designed to satisfy, in a single piece of apparatus, the principal needs of auditory research in a psychological laboratory. A single device, capable of generating tones of any desired frequency, intensity, and complexity, and of presenting the tones to an *O* in a variety of combinations by means of simple dial-controls, will serve as the nucleus of almost any experiment in psychological acoustics. Hence, we have devised a twin-oscillator which, when used to activate a loud speaker or ear-phone, generates two tones, whose dimensions can be separately controlled, and which can be presented either successively or simultaneously.

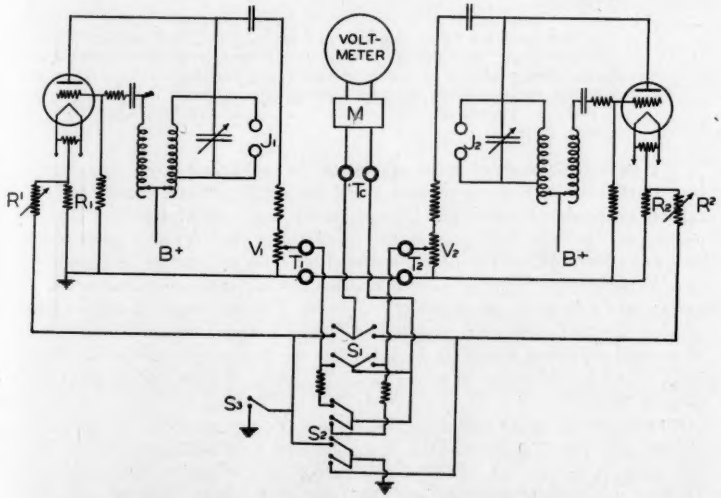

FIG. 1. SCHEMATIC DIAGRAM OF TWIN-OSCILLATOR

The plate potential for both oscillators is supplied by a rectifier and filter, not shown in the diagram. Hence the instrument is entirely a.c. operated.

Some of the typical experimental problems to which the twin-oscillator would be adequate are as follows.

(I) Problems demanding the successive presentation of two tones which differ in frequency, intensity or both.
  (1) Just noticeable differences for pitch.
  (2) Just noticeable differences for loudness.
  (3) Isophonic contours; the values of frequency and intensity which two tones must have in order to be perceived as equal in respect of some psychological attribute. These contours can be determined for (a) loudness, (b) pitch, (c) volume, and (d) density.
  (4) Comparison of pitch as perceived by the two ears separately. One tone is led to one ear and another tone to the other ear. The frequency is adjusted until the pitch appears equal at the two ears.
  (5) Fractionation of tones: finding the values at which one tone appears to be a certain fraction of another tone with respect to pitch, loudness, volume or density.

(II) Problems demanding the simultaneous presentation of two tones.
  (1) Beats: produced by allowing the oscillators to beat electrically, or by allowing the tones generated in separate speakers to beat acoustically.
  (2) Binaural beats: produced by leading a slightly different frequency to each ear.
  (3) Difference tones, summation tones, and other combination tones.
  (4) Phenomena of masking.
  (5) The magnitude of aural (subjective) harmonics. One tone is allowed to beat with the aural harmonic of another tone and the intensity is determined at which the beats are most pronounced.
  (6) Binaural localization as a function of stimulation by a different intensity at each ear.
  (7) Binaural localization as a function of phase differences at the two ears. The two tones are made identical in frequency and their relative phase is adjusted to any desired value. This adjustment is effected by changing the frequency of one of the tones by a fraction of a cycle per sec. and allowing the two tones to beat (change phase) until the desired phase is reached. Thereupon the frequency is made identical again and the beating stops.

The apparatus consists of two oscillators of the tuned plate-circuit type mounted in a single cabinet. Both the frequency and the output voltage of each oscillator can be independently controlled. Changes in frequency are obtained by changing the capacity and the inductance of the tuned circuit, and for this purpose each oscillator is provided with a decade condenser of three sections. In addition to the decade condenser, a large air condenser makes possible continuous changes in frequency over a range of one or two cycles per sec. The total range of each oscillator is from 150 to 14,000 c.p.s., and can be further extended downward by the use of external capacitors, which can be added into the circuits by means of the plug-in jacks, J_1 and J_2. The output of each oscillator is separately controlled by the potentiometers, V_1 and V_2.

Three sets of output terminals, T_c, T_1 and T_2, are connected in the output circuits. One pair, T_c, is connected in parallel with both oscillators, and the other two pairs, T_1 and T_2, are connected separately to each oscillator. The combination of these three sets of terminals and the switches, S_1, S_2 S_3, make possible three output arrangements as follows:

(1) With switch S_2 open, the output of either oscillator can be applied to the terminals T_c by turning the selector switch S_1. When switch S_1 is turned to the

right, the second oscillator is connected to T_e, and when S_1 is turned to the left, the first oscillator is connected to T_e.

(2) With the switches S_1 and S_2 open, and with S_3 closed, the first oscillator is connected to T_1 and the second is connected to T_2. Hence, two loud speakers can be activated simultaneously, each at a different frequency.

(3) With switch S_1 open and with S_2 closed, both oscillators are connected to T_e, so that they beat electrically. The oscillators will beat at very slow rates without locking step with each other.

In order to reduce the click which occurs when a potential is applied suddenly to a loud speaker or ear-phone, the switches are made to turn the oscillators themselves on and off as well as to select the output circuits. The oscillators are turned on and off by changing the grid bias on the oscillator tube. The grid-bias potential across the resistors, R_1 and R_2, is sufficiently negative to prevent oscillations. However, on turning the appropriate switch—S_1, for example—a resistance is connected in shunt with the grid-bias resistors so that the resulting parallel resistance of the combination becomes low enough to reduce the bias potential and start oscillations. The values of the shunting resistors, R^1 and R^2, can be so adjusted that the oscillators will build up slowly enough to eliminate the click. The controls for varying R^1 and R^2 are mounted on the front panel and can be readily adjusted. This adjustment also effectively controls the harmonic content of the output.

A rectifier-type, A.C. voltmeter is connected across the output terminals, T_e, in such a way that the voltage across either of the output circuits can be measured. This meter is provided with a multiplier, M, which makes it possible to cover the entire voltage range of the oscillators. An off position in the multiplier switch can disconnect the meter when desired.

The oscillators are mounted in a single standard cabinet 19 in. wide and 17 in. high. All controls are on the front panel and are conveniently placed so as to minimize fatigue on the part of *E*. This instrument can be built for less than $200, including the labor.

AN APPARATUS FOR CONTROLLING A STOP WATCH BY MEANS OF SOUND OR LIGHT

By NORMAN L. CHALFIN, University of Georgia

The apparatus whose wiring diagrams are shown in Figs. 1 and 2 was devised to control a stop watch by means of sound or light. The principal part of the apparatus is a 6H6 metal tube which is used for rectification of audio frequency voltages. The direct current thus produced by sound operates a series of relays to control a stop watch.

An amplified audio signal is stepped up at the output of the audio amplifier through a transformer with a center-tapped secondary. Rectification of the audio voltages is accomplished with the 6H6 tube and the rectified voltage used to operate a sensitive moving coil relay (Weston Model, No. 634) which in turn controls a secondary relay that starts or stops the watch.[1]

[1] The Stoelting electrical stopwatch controller (No. 20219) is the device we used.

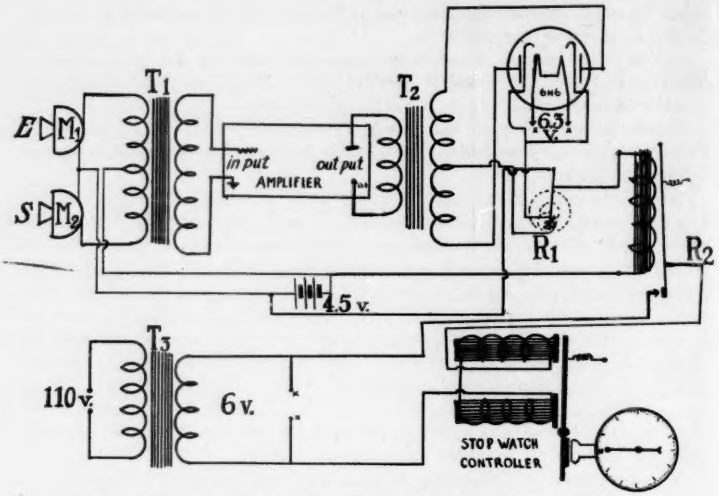

FIG. 1. WIRING DIAGRAM FOR SOUND-CONTROLLED STOP WATCH

Fig. 1 shows how two single button microphones are connected across a double button microphone transformer (T_1). A 4.5 v. battery which supplies voltage for the secondary relay (R_3) is also used to supply the current for the two microphones. If a 6 v. transformer is used for the magnet controlling the stop watch the filament voltage (6 v. A.C. or D.C.) for the 6H6 tube may be taken from that source.

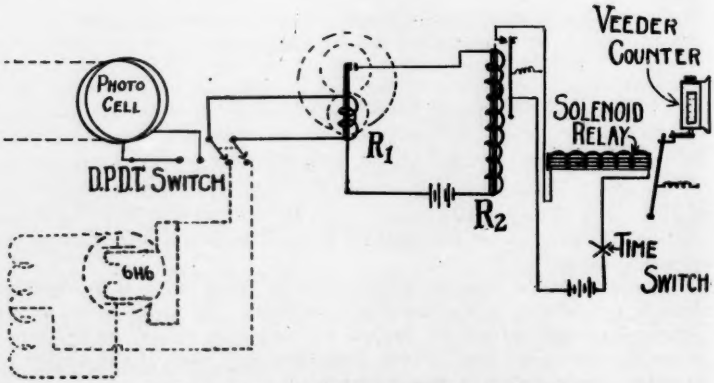

FIG. 2. WIRING DIAGRAM MODIFIED FOR USE OF PHOTRONIC CELL AND VEEDER COUNTER

In using the apparatus the *E* sits at M_1 and the *S* at M_2. *E* gives the stimulus-word and the sound of his voice starts the watch. The sound of the *S*'s voice giving the response stops the watch. By pressing a push button, which is connected across the contact terminals of R_3, the stop watch may be reset to zero.

Fig. 2 shows a method of using the same apparatus with a photronic cell as the starting mechanism. A light beam is focused on the cell. When this beam is broken the watch may be started or stopped. Also in this figure is shown a method of connecting a counter so that a photo-electric counter may be had. With a sufficiently sensitive microphone and amplifier, the device may also be used as an automatic heart beat counter by connecting the counter in place of the stop watch. The writer has set a 15-sec. time switch in such an arrangement and multiplied the reading on the counter by 4 to get the heart beat rate while other observations were made.

We have used these devices in the psychological laboratory of the University of Georgia for measuring verbal reaction-time; for determining the human error in the use of a stop watch and other experiments, and have found them eminently satisfactory.[2]

AN INSTRUMENT FOR MEASURING INTENSITY OF PRESSURE AND PAIN STIMULI

By Ray Mars Simpson, Northwestern University

The device shown in Fig. 1 makes it possible to control stimulus-conditions accurately while making measurements of the sensitivity of the skin.[1] Essentially it is a lever arrangement by which the height of fall of a rigid stimulus-point is regulated by micrometer adjustments. The micrometer (1) bears a scale calibrated in units of 0.0001 in. The frame of the micrometer is bolted rigidly in vertical position to a heavy brass base 5½ in. long (2). This base supports a balanced lever ⅛ in. square and 4 in. long (3) suspended by delicate cone bearings. At the end of the balance lever (4) adjacent to the spindle of the micrometer a short steel pin with rounded point is embedded. This pin is held in contact with the spindle of the micrometer by a small coil spring at the opposite end of the balance. The stimulus-point is clamped to the side of the balance arm (5) by means of small bolts, enabling the experimenter to make adjustments as desired. In determining sensitivity to pressure, points of various sizes are used. If painful stimulation is desired, sharper points are substituted.

In the past great difficulty has been encountered in maintaining fixed relationships between the skin area stimulated and the stimulus. In order to obviate this type of error a small brass ring 1 in. in diameter, 3/32 in. in thickness and ½ in. in height is brazed to the end of the supporting base (6) and serves to hold the skin

[2] These devices were developed at the University of Georgia under the supervision of Professor A. S. Edwards.

[1] The writer is pleased to acknowledge suggestions of Dr. G. L. Freeman which led to the construction of this apparatus.

taut as the head of a drum. When the instrument is lowered upon the arm the stimulus-point makes contact with the epidermis in the center of this ring. A winged adjustment screw (7) is provided for raising or lowering the entire stimulus-unit in an iron channel guide (8). This guide is mounted upon a wooden trough 5 x 3 x 20 in. in size.

Uniformly discrete stimuli are presented at approximately 2.14-sec. intervals by means of an automatic activating unit A-B-C. A solenoid (A) operates an arm which alternately depresses and releases the stimulus-lever. The solenoid unit is mounted upon a metal arm by means of a bolt (x) which permits suitable adjust-

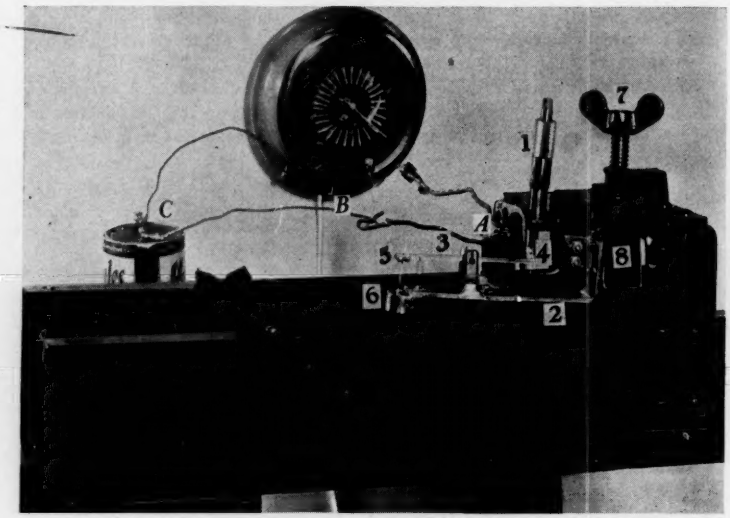

FIG. 1. AN INSTRUMENT FOR MEASURING INTENSITY OF PRESSURE AND PAIN STIMULI

ments.[2] The under surface of the activating arm is cushioned by a thin rubber pad. A special insulated dial attached to the face of an electric clock (B) is connected in series with the solenoid, a dry cell (C) and the frame of the clock. A drag soldered to the minute hand makes contact with the insulated dial thereby closing the circuit, depressing the solenoid lever, and lifting the stimulus-point off the skin. The electrical circuit is broken at short intervals as the drag passes over enamel strips radiating from the center of the insulated dial. Each time the drag passes over one of these enamel strips the stimulus-point makes contact with the skin.

For threshold measurements *S*'s arm should be packed comfortably in the wooden trough by means of rubber sponges. The stimulus unit is then lowered over the

[2] Since the solenoid unit causes a slight vibration it may be desirable to mount it separately.

volar surface. Discrete serial stimulation, in order of either increasing or decreasing magnitude, is made possible by slight turns of the thimble of the micrometer during the intervals when the stimulus-point is lifted clear of the skin. The intervals between stimuli may be varied in length to offset anticipatory responses by altering the spatial separations of the enamel strips on the dial of the clock.

A TESTOMETER

By NOEL B. CUFF, Eastern Kentucky State Teachers College

A new type of apparatus which determines automatically by weight the number of correct answers to an examination and counts responses for certain learning, tapping, and other experiments has been developed by the writer. The device used

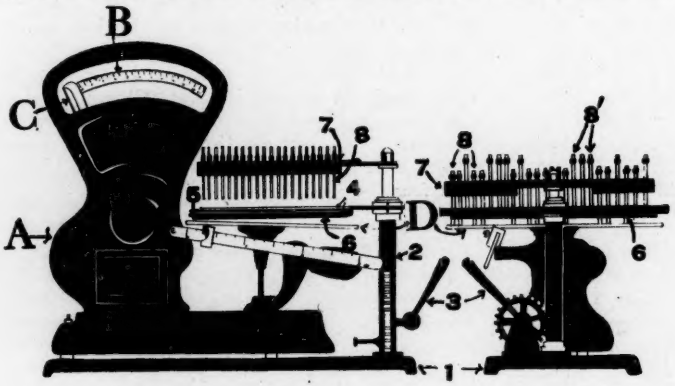

FIG. 1. A TESTOMETER

with a sensitive laboratory scale is called a *testometer*. It (1) mechanically eliminates errors in marking and in counting responses; it (2) does labor in two minutes that it would take a scorer an hour to do; and it (3) saves about 95% of the ordinary cost of giving and scoring examinations.

The procedure is as follows. A perforated answer-card and an objective test, work-book assignment, or an experiment are given to every student. The instructor then tells them to indicate which they consider the correct response to a test question or problem by punching with a pencil in one of the answer-card holes allotted to the given item. At the end of the examination or experiment the cards that have been punched by a group are collected and the reactions are counted by weighing.

Fig. 1 shows that the testometer is simple in construction and fool proof in operation—there being no gadgets to get out of order. It consists of a scale (A) that has ¼ oz. graduations (B) on the face and an indicator (C)—an inexpensive spring scale can be used. The platform of the scale is lettered (D). The

scale rests on the testometer's base (1). At the right, vertical to the base (1), is a rack and pinion gear (2), which supports an open frame (6), similar to a picture-frame without a glass, with a side rail at the back and right to facilitate the placing of the answer-card which consists of two perforated cardboards (4) and a thin sheet of paper (5). Above the answer-card frame (6) is a box-like elevator for weights (7). It is an inch thick, and in length and breadth about the size of the answer-card. In it are 500 holes exactly corresponding to the holes in the answer-card. In setting the apparatus for a particular test the operator inserts, in every hole that corresponds to a correct answer, a stylus or pin (8) which has a head larger than the hole in the elevator (7). The styluses hang supported by their heads when the elevator is up. They are light, ¼ oz., consequently, they will be held up by the answer-card, when the elevator is lowered, unless a hole has been correctly punched—see (8′) in Fig. 1. If a hole has been correctly punched, the stylus will pass through it and will rest on the scale pan and be weighed.

In scoring a paper the operator simply places it on the frame (6) and lowers the elevator (7) with lever (3). At every place where a hole has been correctly punched a stylus passes through the paper and rests on the scale pan (D) which is just below the answer-card. In all other cases the styluses are held up—see (8) and (8′). Since every stylus weighs ¼ oz., the indicator (C) will show the weight of all the styluses on the pan and hence the number of correct answers. For example, if the indicator registers 20 oz. there are 4 x 20 or 80 correct answers; likewise, if it stands at 25¼ oz. there are 101 correct answers. The score is always the number of ounces times four.

NOTES AND DISCUSSIONS

Real Movements, Apparent Movements, and Perception

It has been said that "the crucial study which began the entire movement of Gestalt Psychology"[1] was that in which Wertheimer suggested an explanation for the phenomenon of apparent movement. When separated by certain temporal and spatial intervals, stimuli alternately presented seem to move from one to the other position. As the physiological correlate for this apparent movement, which he labeled "the phi-phenomenon," Wertheimer postulated a short-circuiting process in the cerebrum.[2] It is probable, however, that the perception of apparent movement, indeed perceptions of all sorts, involve a complete circuit routed through the brain from the original receptors to some pattern of muscle fibers, and back again from the proprioceptors there stimulated to the brain, and so on with constantly shifting pattern. On this assumption, perception differs from overt response in being merely an anticipatory act, symbolic of overt response, carrying or representing the meaning of some completed act or acts. Inasmuch as the responses associated with retinal stimulation may involve any of the striped muscles, there is no reason for limiting one's search for the associated movements to those of the eye. The thesis presented in this paper is that the determination of the muscular mechanisms involved in particular perceptions has been retarded by failure (1) to distinguish the different types of muscular movement, and (2) to note that apparent movements are marked by those characteristics peculiar to but one of these types.

Analysis of movement. Records obtained by oscillograph confirm the older kymograph evidence of the validity of the Beaunis-Richer distinction between ballistic and tense movements.[3] In movement made under tension the antagonistic muscle groups are continually contracted against each other. Equalization of the pulls of the antagonists produces a fixation; the member does not change its position. If the pull of one muscle group is greater than that of its antagonist, the member moves but under tension. In the ballistic movement, on the other hand, the muscle action is entirely different. The muscle groups do not contract against each other. The muscle group which drives the muscle contracts suddenly and relaxes immediately.[4] As both muscle groups are now relaxed, the member moves forward under momentum.

Similarity between ballistic and apparent movements. (1) These ballistic, thrown, movements may be terminated in one of three ways: (1) by an opposing surface, as in keyboard playing or typing, (2) by the contraction of an opposing muscle group, as in rapid bowing of a violin, or (3) by the passive stretch of muscles and ligaments, as in the follow-through of the golf stroke. In the second and third cases, since the only control exercised over the ballistic movement is that given in the initial impulse, the movement occasionally falls short of its mark, or travels beyond it.

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[1] E. G. Boring, *A History of Experimental Psychology*, 1929, 571.

[2] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 61, 1912, 161-265.

[3] K. Wachholder, Willkürliche Haltung und Bewegung, *Ergeb. d. Physiol.*, 26, 1928, 568-775. R. H. Stetson and H. D. Bouman, The coördination of simple skilled movements, *Arch. neerl. de physiol.*, 20, 1935, 177-254.

[4] Stetson and Bouman, *op. cit.*, 200.

A similar pattern is found to occur in apparent movement, particularly emphasized in the work of Scholz,[5] Higginson,[6] and Neuhaus.[7] Scholz, who worked with auditory and tactile, as well as with visual material, found that when two stimuli are separated by small distances, in each of these sense modalities the phenomenal movement is overextended. When the stimuli are presented at greater distances his *Os* usually reported that the apparent movement fell short of the actual distance between stimuli.[8] Neuhaus obtained an estimate of the amount of apparent overshooting. Time-intervals were so arranged that one pair of stimuli were seen simultaneously while the other pair produced stroboscopic movement. The distance between the first pair was then increased until *Os* reported it to equal the length of the apparent movement. The *Os* displayed appreciable individual differences in the amount of overextension characterizing the apparent movement and this amount remained practically constant for each *O*, when the distance between the stimuli was increased from 6.5 cm. to 11 cm., and then to 16 cm. It would be interesting to correlate this constant tendency with the amount of follow-through characterizing their overt movements, as, for instance, their cursive handwriting.

(2) Ballistic and tense movements can be distinguished by their kymograph records and variation in velocity. Even when a member is moved rapidly under tension, its record, as seen on the kymograph, is marked by tremors, whereas the ballistic tracing is featured by the straight-edge character of its momentum phase. A second characteristic of the ballistic movement is its cursiveness. Whereas tense movements mark the sharp angle made by a change in direction, momentum movement takes the turn cursively, has a tendency to 'cut the bases.' This is evident in handwriting, beating time, swinging the bat, racket, etc.

Investigators have frequently noted that apparent movements show this cursive feature. In reporting her study done in Koffka's Giessen laboratory, in which stimuli were presented in the neighborhood of the blind-spot, Stern said that as a rule the moving line was seen as "a curved bow."[9] Korte quoted a representative introspection of one of his *Os*, as follows: "One has the impression that the line makes a movement in a circle."[10] In one arrangement of the stimuli involving two sets of three lights, Neuhaus[11] presented one group of lights which marked the apices of a triangle resting on its base, alternating with a group whose imaginary base-line is above. His *Os* reported that arc-shaped phi-movements were perceived, which swung around like a child's jumping rope. Mibai[12] found that straight lines were commonly perceived as curved, when presented cinematically. In one part of her study, she presented a figure consisting of six radii, resembling spokes of a wheel 60° apart. Alternate

[5] W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, *Psychol. Forsch.*, 5, 1924, 219-272.

[6] G. D. Higginson, The visual apprehension of movement under successive retinal stimulation, this JOURNAL, 37, 1926, 63-115.

[7] W. Neuhaus, Experimentelle Untersuchung der Scheinbewegung, *Arch. f. d. ges. Psychol.*, 75, 1930, 315-458, esp. 386-400.

[8] Scholz, *op. cit.*, 268.

[9] A. Stern, Die Wahrnehmung von Bewegung in der Gegend des blinden Flecks, *Psychol. Forsch*, 7, 1925, 10.

[10] A. Korte, Kinomatoskopische Untersuchungen, *Zsch. f. Psychol.*, 72, 1915, 202.

[11] Neuhaus, *op. cit.*, 438.

[12] S. Mibai, An experimental study of apparent movement, *Psychol. Monog.*, 42, 1931, (no. 190), 1-91.

frames presented the spokes with positions shifted 30°. The spokes first appeared to bend counterclockwise, on the left, and clockwise on the right of the vertical diameter. For 10 of the 15 *Os* the radii were seen to assume cursive shape.

We may recall the tachistoscopic studies which lead to the formulation of 'the law of pregnancy' according to which *Gestalten* tend to complete or to emphasize their natural form. The gamma or expansion-movement, to which Lindemann in particular addressed his attention, has the characteristics of the ballistic movement. When, for example, he presented thimble-shaped (*zuckerhutiges*) figures, his *Os* reported a strongly marked tendency for the base-line to extend itself and for the figure to assume a contour approximating that of the normal frequency curve.[13] Moreover, if there are small gaps in the outline-figure presented, "the free ends show the most pronounced tendency to close together forming a complete figure."[14] A contour outlined in dots is filled in by imaginary lines. Thus a twelve-pointed figure was always reported to be seen as a circle, never as a dodecagon.[15]

(3) Ballistic movement is also characterized by the fact that its duration is roughly independent of the length of its excursion.[16] When marking time with a baton, for example, one may vary the length of the beat stroke without altering the tempo. One writes a large hand as rapidly as a fine hand.

This phenomenon also appears in the phi-movement. Neuhaus was surprised to find that, when the time interval was fixed and the distance between the first and second stimulus-lights was altered, there was a change in the speed of the apparent movement with an increase in the distance. The stimuli were first presented 3.75 cm. apart, then separated by 7.5, 11.25 and 15 cm. With the time-interval held constant, every *O,* without exception, reported the apparent movement as being, for the different distances respectively, slow, easy, more rapid, and finally "as swift as lightning."[17]

Illusions of movement aroused upon tactual and auditory stimulation also have the characteristics of the phi-phenomenon visually stimulated, as is evident in the work of Benussi, Burtt, Scholz, Hulin, and Neuhaus.[18] This fact strongly suggests that explanation for the illusion may well be sought in the factor which is common to all of its manifestations, viz., in the response, rather than in the stimulus, phase of the pattern. This suggestion seems especially pertinent because, in the tactual and auditory, as well as the visual field, apparent movements have the characteristics of ballistic muscular contractions.

Antagonism of tense and apparent movements. The descriptive reports furnish

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[13] E. Lindemann, Experimentelle Untersuchungen über das Entstehen und Vergehen von Gestalten, *Psychol. Forsch.,* 2, 1922, 5-60, esp. 27-28.

[14] *Op. cit.,* 28.

[15] *Op. cit.,* 42.

[16] R. H. Stetson, Motor theory of rhythm and discrete succession, *Psychol. Rev.,* 12, 1905, 261; M. Isserlin, Über den Ablauf einfacher willkürlicher Bewegungen, *Psychol. Arb.,* 6, 1910, 86; F. N. Freeman, Analysis of writing movements, *Psychol. Monog.,* 17, 1914, 1-46; Stetson and Bouman, *op. cit.,* 229.

[17] Neuhaus, *op. cit.,* 405.

[18] V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psychol.,* 36, 1917, 59-135; H. E. Burtt, Tactual illusions of movement, *J. Exper. Psychol.,* 2, 1917, 371-385; Scholz, *op. cit.;* W. S. Hulin, Apparent tactual movement, *J. Exper. Psychol.,* 10, 1927, 293-320; W. Neuhaus, Taktile Scheinbewegung, *Arch. f. d. ges. Psychol.,* 83, 1932, 519-562.

just as strong evidence for the negative, as for the positive, aspect of the thesis. Tense movements are inimical to the phenomenon of apparent movement.

Benussi, while noting how the tendency of lines to bend and curve, when arranged in Müller-Lyer and Zöllner figures, is exaggerated when the figures are moved at certain speeds, laid special stress upon the statement that apparent movement depends upon having a "synthetic" attitude, and that an "analytic" attitude dissipates the illusion.[19] This result was also reported by Koffka and Kenkel,[20] and by Bates.[21] Ehrenstein, whose work with visual illusions confirms Benussi's, found that the fixation of a central point in figures which otherwise induce illusory movement tends to dispel the illusion.[22] In fact, this was made evident in the work of Judd, thirty years ago, who said that "an analytical examination" of the Müller-Lyer figure, characterized by pauses, was "decidedly typical" of the eye movements of the *O* who continued a practice series until the illusion disappeared.[23]

The work of Lewis with the Müller-Lyer illusion and with filled and unfilled space supplements the other studies in a manner which is very significant. He found that practice produces a marked decrease in the illusory effect, only when *O* is permitted to make a prolonged examination of the figures; if *O* is permitted only a "momentary" glance—the length of the exposure was not reported—practice produces very little change in the illusion. In other words, the persistence of the illusion is associated with that type of motor adjustment which one tends to make when the exposure is very brief.[24]

Mibai's work also shows that the distortion appearing in the perception of straight lines, presented cinematically, depends upon the nature of the attentive adjustment. As reported in one representative introspection, when one "looked at the whole thing 'harder,' the bars became straight, and the bending occurred when she was not looking 'so hard'."[25] Guilford said that "following the stimuli voluntarily with the eyes tends to destroy the phi-phenomenon."[26]

Hillebrand found that *Ganzbewegung* (a movement which extends the entire distance) between the two stimuli was in general produced by a wandering glance; with fixation there appears only "an empty space." He especially emphasized the observation that fixation of the point where *O* expects the second light to appear "injures the stroboscopic movement" or abolishes it entirely.[27]

Neuhaus reported that, by fixing a point in the middle of the field under conditions which would otherwise be favorable to apparent movement, he can "destroy the apparent movement." "I see then not one point, which moves, but two points,

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[19] V. Benussi, Stroboskopische Scheinbewegungen und geometrische optische Gestalttäuschungen, *Arch. f. d. ges. Psychol.*, 24, 1912, 31-62.

[20] K. Koffka, and F. Kenkel, Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse, *Zsch. f. Psychol.*, 67, 1913, 422, 437.

[21] M. Bates, A study of the Müller-Lyer illusion, this JOURNAL, 34, 1923, 46-72.

[22] W. Ehrenstein, Versuche über die Beziehung zwischen Bewegungs- und Gestaltwahrnehmung, *Zsch. f. Psychol.*, 96, 1925, 305-352.

[23] C. H. Judd, The Müller-Lyer illusion, *Psychol. Monog.*, 29, 1905, 72.

[24] E. O. Lewis, The effect of practice on the Müller-Lyer illusion, *Brit. J. Psychol.*, 2, 1908, 294-306; Illusion of filled and unfilled space, *ibid.*, 5, 1912, 36-50.

[25] Mibai, *op. cit.*, 63.

[26] J. P. Guilford and H. Helson, Eye movements and the phi phenomenon, this JOURNAL, 41, 1929, 599.

[27] F. Hillebrand, Zur Theorie der stroboskopischen Bewegungen, *Zsch. f. Psychol.*, 89, 1922, 238.

which appear either simultaneously or in succession." Neuhaus also noted the results which followed when an *O* was instructed to direct his glance toward one of the two points. Although the temporal and spatial conditions were such as, without instructions to fixate a particular point, had commonly resulted in apparent movement, the *O*s now reported no such movement.[28]

Lindemann also found very little gamma movement in the neighborhood of a point that was fixated, while such apparent movement was intensified in the most distant portions of the figure. This phenomenon was much more characteristic of contour-figures than of surface-figures.[29] The various phrases which appear in the reports—"analytic" as contrasted with "synthetic" attitude, "looking at the thing harder," "following the stimuli voluntarily", "to direct one's glance toward one point", "a fixed glance"—were not submitted as objective evidence, but only as hints which point to a consistent relationship between tense movements and the absence of the phenomenon of apparent movement.

The time factor. One of the chief obstacles to the credibility of the theory that movements play a rôle in perceptive processes has been the impression that muscular contractions require too much time. The phi-phenomenon usually appears within a span of 100 millisec. Wertheimer found the interval between exposures to be optimal at about 60 millisec., the length of exposures being approximately 10 millisec.[30] The later studies, which have shown that apparent movement may be elicited with a zero-interval between stimuli (Hillebrand, Higginson, Van der Waals and Roelofs),[31] indicate that the essential condition is the sudden appearance of a second effective stimulus, when one has been regarding the first which has disappeared. The exposure-times which have been found effective for producing apparent movements are, to be sure, too brief to permit the execution of tense movements, those characterized as 'controlled' or 'voluntary,' such as are presumably involved in the 'analytic attitude.' They are, on the other hand, not beyond the speed of ballistic movements. According to Dodge's measurements, saccadic (ballistic) movements of the eye through an angle of 5° may require less than 25 millisec.[32] Oscillographic photographs of action currents from muscular contraction show that the time required to give the impulse for a ballistic movement is usually from 45 to 50 millisec. but may be as brief as 25 millisec.[33] The maximum repetition time for tapping with the fingers is about 9-10 per sec. As each such movement involves a back-stroke as well as a beat-stroke, this means 18-20 innervations, and allows approximately 50 millisec. for each one. Large muscle groups require longer innervation periods; the ballistic pulse for lateral flexions of the trunk require about 250 millisec.[34] One might note, in passing, that apparent movement induced by tactual stimuli separated by relatively long distances are associated with longer temporal intervals than those induced by retinal stimulation.[35]

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[28] Neuhaus, *op. cit.*, 423. [29] *Op. cit.*, 35-37. [30] Wertheimer, *op. cit.*, 179.

[31] Hillebrand, *op. cit.*, 244; Higginson, *op. cit.*; H. G. van der Waals and C. O. Roelofs, Optische Scheinbewegung, *Zsch. f. Psychol.*, 114, 1930, 241-288; 115, 1931, 91-193.

[32] R. Dodge and T. S. Cline, The angle velocity of eye movements, *Psychol. Rev.* 8, 1901, 145-157.

[33] Stetson and Bouman, *op. cit.*, 209.

[34] Unpublished records in the Oberlin Psychological Laboratory.

[35] Neuhaus, Taktile Scheinbewegung, *Arch. f. d. ges. Psychol.*, 83, 1932, 519-562.

Summary. Because of the fact (1) that the temporal factors conducive to the appearance of apparent movements correspond to those of the impulse which initiates the ballistic movement, but are too brief for tense movements; (2) that the introduction of tense movements or fixations is so generally reported as inimical to the phi-phenomenon; and (3) that apparent movements have the leaping or sweeping, over-shooting and cursive characteristics of the momentum movement, their duration being independent of their length, it seems probable that *ballistic contractions in fibers of some of the finer muscles occur at the time when the phenomenon of apparent movement is experiencd.*

Oberlin College L. D. HARTSON

A NOTE ON 'ADVANCING AND RETREATING' COLORS

Many individuals have noted that different colors, even when in the same plane, seem to be at different distances. In 1918, Luckiesh reported a brief experiment in which 9 observers were asked to equate the apparent distances of a red X and a blue E by varying the actual distance.[1] Seven of the 9 *O*s needed to have the red more remote if the two were to seem at the same distance, one must have the blue more remote, and one was inconsistent or did not show the illusion at all. For him the differences were slight.

In a second experiment by Sophie Belaiew-Exemplarsky,[2] the distance of illuminated bits of Hering papers were compared and equated as to distance by moving the one that seemed nearer back until it seemed to be as far away as the other. A reducing screen before the paper decreased the specific surface effect of the color. The long wave colors all seemed nearer than the short wave colors. Red and blue showed especially striking differences. No mention is made of individual differences and specific values are not given for the different *O*s.

Similarly Katz found that if he drove a series of broad-headed nails into a board and then covered alternate nails with red and blue papers, the red papers seemed, when viewed from a distance of 80 cm., to be strikingly nearer (about ½ to 1 cm.) than the blue.[3] The results were reversed in reduced illumination and with a dark adapted eye. Katz assumed that the illusion depends upon the 'Eindringlichkeit' of the colors. The more penetrating, or, as he translates it, insistent, the nearer seems the color. All three of these earlier observations assert that red always seems nearer than blue, although Luckiesh found one definite exception to the rule.

The present investigation is a study of the apparent nearness of red as compared with blue. The apparatus used was devised by J. F. Shepard for Woodburne's study of the perception of depth. The apparatus afforded a means of comparing the red from a neon light with the blue from argon and neon under circumstances in which the size of the light was compensated at different distances to give the same retinal image, no matter what the distance. An inclined rail below the supporting rails pushed up a wheel that was connected with levers which in turn shortened and narrowed the slit through which the lights were seen. As the container was

[1] M. Luckiesh, On "retiring" and "advancing" colors, this JOURNAL, 29, 1918, 182-186.

[2] S. Belaiew-Exemplarsky, Über die sogennannten "hervortretenden" Farben, *Zsch. f. Psychol.*, 96, 1925, 400-429.

[3] David Katz, *The World of Colour*, 1935, 69 f.

brought nearer the eye, the slit shortened and narrowed just enough to nullify the normal increase in size with approach to the eye. The lights were from tubes containing the gases that were mounted in the light-tight boxes. One of the tubes which in Woodburne's experiment had both contained neon was replaced by a tube filled with an argon and neon mixture. Both containers could be moved on parallel rails. The details of the construction may be obtained from Woodburne's article.[4]

TOTAL NUMBER AND PERCENTAGE OF JUDGMENTS
(E=equal; B-N=blue nearer; R-N=red nearer; T=total.)

Os	Judgments	Equidistant				Blue 100 cm. nearer				Red 100 cm. nearer			
		E	B-N	R-N	T	E	B-N	R-N	T	E	B-N	R-N	T
A	N	270	87	93	450	180	72	48	300	185	45	100	330
	%	60	19	21		60	24	16		56	14	30	
B	N	172	264	44	480	101	219	10	330	128	206	26	360
	%	39	55	9		31	66	3		36	57	7	
C	N	77	260	53	390	45	218	37	300	59	187	84	330
	%	20	67	13		15	73	12		18	57	25	
D	N	230	101	29	360	168	86	76	330	144	108	78	330
	%	64	28	8		51	26	23		44	33	23	
F	N	52	194	84	330	42	210	108	360	65	157	108	330
	%	16	59	25		12	58	30		20	47	33	
G	N	135	174	111	420	110	113	77	300	99	111	90	300
	%	32	41	27		37	38	25		33	37	30	
H	N	146	145	129	420	122	82	126	330	135	111	84	330
	%	35	34	31		37	25	38		41	34	25	
I	N	118	155	117	390	106	90	74	270	115	103	142	360
	%	30	40	30		39	33	28		32	29	39	
J	N	168	224	88	480	112	141	47	300	120	212	58	390
	%	35	47	18		37	47	16		31	54	15	
K	N	224	106	90	420	96	185	49	330	216	42	102	360
	%	53	25	22		29	56	15		60	12	28	
L	N	124	270	56	450	36	240	24	300	69	205	26	300
	%	28	60	12		12	80	8		23	68	9	
M	N	271	73	106	450	189	64	77	330	137	20	113	270
	%	60	16	24		57	20	23		51	7	42	
N	N	80	191	89	360	71	176	53	300	41	176	143	360
	%	22	53	25		24	58	18		11	49	40	
P	N	122	241	57	420	116	178	41	330	99	157	74	330
	%	29	57	14		34	54	12		30	48	22	
Q	N	6	273	111	390	14	140	176	330	2	211	87	300
	%	2	70	28		4	43	53		1	70	29	

In this experiment, observation was with one eye alone. The *O* had his head held in a fixed position by a supporter and a biting board. He looked through an artificial pupil adjusted to put his fixation-point between the two lights to be compared. He sat behind a curtain and screen which were illuminated to give the daylight adapted retina. The lights were in a dark room so that only the two

[4] L. S. Woodburne, The effect of a constant visual angle upon the binocular discrimination of depth differences, this JOURNAL, 46, 1934, 273-286.

illuminated slits could be seen through the artificial pupil. The standard distance was 295 cm. and at that distance the slit was 2.5 centimeters high and 0.5 mm. wide. The method of right and wrong cases was used. First the two lights were set at the same distance of 295 cm. and the *Os* were asked to say which seemed nearer. From 350 to 480 judgments were given by every *O* at this distance. Then the lights were set with the blue light at 245 cm. and the red at 345 cm. and from 280 to 360 comparisons made. In a third setting, the red was at 245 cm. and the blue at 345 cm. and approximately the same number of estimates made. Fifteen *Os* carried through the entire series of experiments. They were students in the year course in beginning psychology and were relatively naïve.

The accompanying table gives the results of the different experiments. It will be seen that, contrary to the results of the earlier studies, the blue seemed nearer than the red for 11 of our 15 *Os*. For 2 *Os* (*A* and *M*), red was nearer—but the percentages of judgments in favor of red (2% and 8%) are small. For the remaining *Os* (*H* and *K*), the difference in favor of blue is so slight (but 3%) that the results may be regarded as equal. These results obtain when the two colors are at objectively equal distances.

It is still more striking that when one color was 100 cm. nearer than the other, the illusion still persisted although for fewer individuals and in some cases in smaller percentages. When the red was at 245 cm. and the blue at 345 cm., 11 *Os* still reported the blue to be nearer, and two of these (*A* and *M*) were *Os* who had reported the red nearer when the two were equal. Of the four remaining *Os*, one (*K*) had given almost the same number of judgments of red as nearer in the first experiment, and another (*I*) had 40% of his judgments for blue as nearer compared with 30% for red nearer in the equidistant series.

When blue was set at 245 cm., and red at 345, 3 of the *Os* reported that red was nearer. One of these (*M*) had given a greater percentage of judgments of red nearer when the distances were equal, one (*H*) had given nearly an equal number of judgments for each color, and the other (*Q*) had favored the blue decidedly, 70% for blue against 28% for red. One *O* (*M*) alone was consistent in all experiments in assigning red the nearer position. Another *O* (*A*) gave red the nearer position, when it was actually nearer and when the two were equally distant, the others saw blue as nearer whatever the actual distances or favored the red only sporadically or when it was actually 100 cm. nearer. It should be remembered that the change in size with distance which is an important factor in discriminating distance under usual circumstances was not an aid in this instance, for the actual size of the slit changed to exactly compensate for the influence of distance on the size of the retinal image.

The explanation of the phenomenon raises many questions. Two general groups of theories have been applied. One would refer it to the chromatic aberration in the refractive media of the eye. The other sees it as an immediate effect of the color quality. The first group uses either the chromatic aberration itself in the single eye or the development of a retinal disparity analogous to the stereoscope from the asymmetrical position of the dispersion circles in the two eyes. Einthoven, in 1885,[6] showed that the red dispersion circle would give crossed disparity when the blue was fixated and the blue dispersion circles would give uncrossed dis-

[6] W. Einthoven, Stereoscopie durch Farbendifferenz, *Arch. f. Ophthalmol.*, 31, 1885, 211-238.

parity. He also showed experimentally that when small artificial pupils are brought before the eyes, the red image can be made to advance by increasing the distance that separates the artificial pupils. Luckiesh repeated the experiment apparently without knowing Einthoven's results, and reaches the same conclusion as to the probable theory.[6] Einthoven found one man for whom the blue seemed nearer and found that for him the directions of the lines of sight were also different from what they were for the other *O*s, due to the position of the eye-balls.

These considerations would not apply to our experiments with monocular fixation. Chromatic aberration alone would be effective. If appreciation of distance were based on interpretation of strain of accommodation, a greater strain would be necessary to obtain a clear image of the red than of the blue, since red is less refracted. Red would then seem to be nearer. This would not explain our results except for the one man who saw red nearer. If the interpretation of distance is not directly dependent upon the degree of strain but is an inference from the unclearness of the image, chromatic aberration might be the beginning of the explanation. Van Tuyl found in her experiments, in which both lights were from neon tubes, that most *O*s were uncertain at first which was more remote, but could say that the two were at different distances.[7] With practice some could decide correctly which was nearer. Blue and red cannot be seen clearly with one adjustment of the lens. That one is seen in dispersion images when the image of the other is clear shows that they are at a different distance. Which is nearer must be decided by chance, or on the basis of some cue from the colors, or from incidental factors. A decision must then be explained by a theory from the second group.

The two most definite suggestions from the quality of colors is that colors that are more intense or in Katz's term, more *eindringlich*, are seen as nearer. We equated the two lights as to intensity as accurately as is possible with lights of different quality. After the results of the comparisons had accumulated, we tried a few observations as to apparent distance with reduced intensity of the blue obtained by placing exposed photographic film behind the slit through which the blue was seen. A considerable objective reduction of the intensity of the light had no effect upon the advancing character of the blue light. It would seem from this that the illusion did not depend upon the relative intensity of the two colors.

Eindringlichkeit cannot be dismissed so readily, but is perhaps more difficult to define, especially in a way that would distinguish it from the 'quality that makes colors seem near.' Such a definition would constitute a tautology. Katz defines 'insistence' (his translation of Eindringlichkeit) as "the power of catching the eye more readily and of holding it more steadily."[8] Belaiew-Exemplarsky uses the term *Grellheit* to describe the quality that makes a color advance. *Grellheit* is said to carry with it a quality which can be called *Eindringlichkeit*. The two are not identical. Grellheit means the strength with which light affects—stimulates physiologically. If *Eindringlichkeit* means the ability to claim attention, it is not *Grellheit* alone, for claiming attention depends also upon subjective moments, such as awakening interest, or being pleasant or unusual. Katz quotes Metzger as saying: "Insistence is not built secondarily upon simple experiences of brightness, but is itself the more primitive experience." All these definitions go little farther than to

[6] Luckiesh, *op. cit.*
[7] M. C. van Tuyl, Monocular perception of depth, this JOURNAL (in press).
[8] Katz, *op. cit.*, 108.

say that colors vary in some quality which we may call insistence and that this quality affects the apparent distance to which the color will be referred.

If we assume that chromatic aberration prevents two different colors from being seen clearly with one adjustment of the lens and so from being referred to the same distance, it may be further assumed that a special characteristic of the color determines which is assumed to be nearer. Red seems to be the nearer color for most of the persons investigated by earlier workers. For 11 of our 15 *O*s, and under the conditions of our experiment, blue seems nearer. In all experiments there is mention of one or more individuals who depart from the rule. We had one who was consistent in seeing red nearer and two more who saw it nearer or about equally distant in two of the three experiments. On the whole, our *O*s were as consistent in seeing blue nearer as the other groups of *O*s were in seeing the red nearer.

The introspections of the *O*s contained no special indication of the cue they used in deciding which color was nearer. Almost all insisted that one color seemed to be projected from the common plane. It just seemed nearer and they could not say how. Incidentally all of the *O*s mentioned the greater brightness of the color that they judged to be nearer as the reason for the statement. All but four also mentioned greater size as the cue for nearness. For 3 of the *O*s size was rather incidental and was rarely mentioned. Most of those who mentioned size would say that one line seemed longer and the longer was assumed to be nearer. Four *O*s asserted that clearness or distinctness formed the cue for discrimination. None mentioned anything that would correspond to insistence. That probably is not to be expected of naïve *O*s from an elementary class. One might use this lack of mention as an argument against the presence of insistence as an immediate element in the naïve perception of color. To this the advocates of insistence might answer with justice that many of the other descriptive words used really involved *Eindringlichkeit* and that if the attribute were pointed out to them and named, they would always see it or at least name it. There is no use laboring either side of the argument until the experiment can be tried.

The mention of size suggests another possible empirical interpretation. Colors do vary in apparent size from the different degrees of irradiation. A bright light irradiates farther than a dark one. Hence the electric light filament when glowing is apparently many times wider than when cold. Red spots on a blue ground seem larger for most people than blue spots of identical size on a red ground. This, by the law of perspective, would explain the greater nearness of the red reported by many people. If this explanation is to hold, it would follow that for our *O*s blue would be the more widely irradiating color, in place of the red as for the others, or under the conditions of their experiments. A more detailed experimentation on this point is necessary before it can be used as an explanation.

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ISOCHROMATIC CONTOURS

There often appears in science a contradiction between conventional generalizations and specific knowledge, with the resulting discrepancy sponsored unwittingly by the same persons throughout a long period of time. For instance, the Epicurean theory of perception, that objects give off impalpable images of themselves and that the images are conducted by the nerves to the brain where the sensorium

perceives them—this theory could not be explicitly supported by any physiologist of the eighteenth century, because a careful consideration of nervous action in those days fixed attention upon the nature of the nerves as conductors of vibrations or animal spirits or some other neural event. Thus both Sir Thomas Young (1801) and Sir Charles Bell (1811) took it for granted that perception depends immediately upon what happens in the sensory nerves and only indirectly upon the nature of the stimulus-object. At the same time, however, the image theory of perception still persisted, so that Johannes Müller was constrained to wage war upon it in order to bring within the range of conventional thinking the notion that the sensorium perceives, not the properties of objects, but the states of the sensory nerves. It is not far wrong to say that Müller (1838) had to refute the image theory a century after its inadequacy had become obvious,[1] nor is it yet certain after another century that the theory has been ostracized from expert psychological thinking.[2] Thus slowly does discovered truth introduce itself into the body of conventional knowledge.[3]

We seem now to be meeting a similar lag in respect of the doctrine of the dependence of sensory attributes upon the dimensions of the stimulus. The old doctrine (close kin of the image theory of perception!) was that every sensory attribute corresponds to some specific dimension of the stimulus: pitch to frequency, hue to wave-length, loudness or brilliance to energy. There is, of course, no such simple relation. It has been shown, furthermore, that the number of attributes is independent of the number of effective dimensions of variation in the stimulus.[4] There may be more attributes than there are stimulus variables, in the way that pitch, loudness, volume, and density all depend upon both the frequency and intensity of a tonal stimulus.[5] In photopic vision there are three attributes (hue, brilliance, and saturation) and only two necessary stimulus-dimensions (wave-length and energy), although complexity is often added as a third dimension, as if the count had to be kept even, three and three. (*Vide infra* for the elaboration of this matter.) In scotopic vision there is only one attribute, brilliance, and its value is a function of both wave-length and intensity. It also varies with complexity, since visual energies summate. Thus all the evidence lies against the conventional view and almost everyone who has any interest in this problem knows the evidence. Nevertheless the conventional view persists in conversation, in text-books, and in the implications of technical writing.

For this reason I venture here to bring together the essential data for color vision. The case of tone has already received attention. In an audiogram whose dimensions

[1] Cf. E. G. Boring, Psychophysiological systems and isomorphic relations, *Psychol. Rev.*, 43, 1936, 565-587, esp. 575-579.

[2] At least Köhler's objection to the "constancy hypothesis" echoes Müller's: Wolfgang Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Zsch. f. Psychol.*, 66, 1913, 51-81.

[3] A. L. Lowell, An example from the evidence of history, *Harvard Alumni Bulletin*, 39, 1936, 208-214, has argued that men change their institutions far in advance of their recognition of these changes in conventional knowledge. For instance, the doctrine that the executive and the legislative branches of the government must be kept separate was formally believed to be a fundamental British principle for many years after it had ceased to be Parliamentary practice. In fact, America borrowed the idea as a successful British principle long after England had actually dispensed with it!

[4] E. G. Boring, The relation of the attributes of sensation to the dimensions of the stimulus, *Philos. Sci.*, 2, 1935, 236-245.

[5] S. S. Stevens, The attributes of tones, *Proc. Nat. Acad. Sci.*, 20, 1934, 457-459.

are frequency and intensity we can plot families of isophonic contours for constant loudness, pitch, volume, and density. The last two sets of contours Stevens has determined,[6] Pitch is kept constant, when intensity is altered, only by a change of frequency—the Zurmühl-Stevens effect.[7] It has long been known how the loudness contours have a minimum in the middle of the audiogram. There are comparable functions for vision, but apparently no one heretofore has troubled to plot the curves on a single 'spectogram.'

Fig. 1 shows the form of the isochromatic contours for color vision. It is a spectogram plotted between wave-length (millimicrons) and illumination (photons). There are really three such diagrams possible. One of them would show a family of contours, each of which would be a line of equal brilliance. The whole system would imply the erecting on the areal base of Fig. 1 a surface whose height indi-

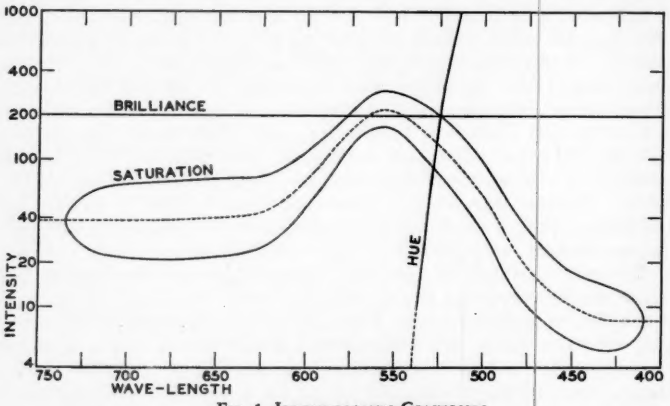

FIG. 1. ISOCHROMATIC CONTOURS

The coördinates are the two dimensions of the stimulus: wave-length (mμ) and intensity (photons). The color for 525 mμ at 200 photons (bright yellow-green) is chosen as the standard of reference, and contours for brilliance, hue and saturation equal to this standard are plotted. The dotted line is the locus of intensities that give maximal saturations at every wave-length (Purdy). Y, G and B indicate the positions, at a middle intensity, of pure yellow, green and blue. Pure red is extra-spectral.

cates the degree of brilliance of every intensity of every wave-length. Thus the isochromatic lines would be the topographical contours of equal brilliances in this surface. Another family of contours implying another surface could be constructed for hue, and a third for saturation. In Fig. 1, however, only three contours—one for each attribute—have been drawn. They show respectively the loci for the

[6] Stevens, Volume and intensity of tones, this JOURNAL, 46, 1934, 397-408; Tonal density, *J. Exper. Psychol.*, 17, 1934, 585-592.

[7] G. Zurmühl, Abhängigkeit der Tönhöhenempfindungen von der Lautstärke und ihre Beziehung zur Helmholtzschen Resonanztheorie des Hörens, *Zsch. f. Sinnesphysiol.*, 61, 1930, 40-86; Stevens, The relation of pitch to intensity, *J. Acoust. Soc. Amer.*, 6, 1935, 150-154.

brilliance, hue, and saturation that are equal to the arbitrarily chosen standard of reference, 525mμ at 200 photons. This stimulus is a blue-green, brighter than the brilliance at which saturation is maximal.

In Fig. 1 the contour for brilliance is a horizontal straight line, passing through 200 photons, because the diagram is plotted against photons. If the figure were translated into radiometric units, these contours would be bell-shaped curves that are concave upward, the reciprocals of the ordinates of the standard visibility curve.

The contour for *hue* is the representation of the Bezold-Brücke effect. This contour is computed from Purdy's data,[8] and it should be noted that the change of wave-length has to be great if constant hue is to be maintained with these large differences in illumination. The contour that passes through 525mμ at 200 photons shifts from 510mμ at 2000 photons to about 538mμ at 10 photons. This is a shift of 28mμ in a region where, at middle intensities, the difference between pure green (505mμ) and pure blue (478mμ) is only 73mμ. But then everyone knows how a good yellow paper on a color-mixer, when mixed with a great deal of black, gives a dark yellow-green that is at least as near green as it is near yellow.

The contour for *saturation* is a closed curve surrounding those intensities for which saturation is maximal. Fig. 1 presents only the general form of it, for some of the necessary data are not available and the exact determination of this function is a problem for future research. The contour shown was plotted in the following manner. Purdy has determined the curve of intensities at which saturation is maximal.[9] This curve, somewhat smoothed, appears as the dotted line in Fig. 1. In topographical terms it is the crest of a ridge in the surface representing saturation. Of course this ridge is not equally high in all places. Jones and Lowry give figures for the saturations of different spectral colors.[10] Their measurements are in j.n.d. and vary from maxima of 23 j.n.d. at the extremes of the spectral range investigated (R of 680mμ and B of 440mμ) to a minimum of 16 j.n.d. in the middle (Y of 575mμ). These measures of saturation must approach zero at the limits of visibility of the spectrum (say, 760 and 400mμ). Such a curve, slightly smoothed, has been drawn, representing the elevations along the crest of the ridge. Perhaps this curve is essentially correct, since Judd has found for yellow that a measure of saturation by j.n.d. is approximately the same as the measure by equal sense-distances.[11] Purdy, on the other hand, has objected that it is obvious that the spectral violets are less well saturated than the spectral reds. These matters are minor: they merely determine the relative width of the closed contour in Fig. 1 at its two ends.

We now have the horizontal projection of the crest of the ridge (Purdy) and the elevations of the crest (Jones and Lowry). To place the contour we need to

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[8] D. M. Purdy, *Chroma as a Function of Retinal Illumination*, Harvard University thesis, 1929, Figs. 10-15, following p. 149. Unfortunately the complete data have not been printed, but see Purdy, Spectral hue as a function of intensity, this JOURNAL, 43, 1931, 548-553 for one function, and L. T. Troland, *Principles of Psychophysiology*, 2, 1930, 169-172 for another.

[9] Purdy, On the saturations and chromatic thresholds of the spectral colours, *Brit. J. Psychol.*, 21, 1931, 283-313, esp. 302-304 and Fig. 3.

[10] L. A. Jones and E. M. Lowry, Retinal sensibility to saturation differences, *J. Opt. Soc. Amer.*, 13, 1926, 25-34.

[11] D. B. Judd, Saturation scale for yellow colors, *J. Opt. Soc. Amer.*, 23, 1933, 35-40.

know the slope and conformation of the sides, but these data are lacking. In the present case I have made the assumption that saturation falls off on either side of the maximum in proportion to the logarithm of the intensitive difference, so that on the semi-log plot of Fig. 1 the upper and lower parts of the contour would be equidistant from the ridge of maximal saturation. This assumption can not be exactly correct, for it supposes that saturation approaches the crest smoothly and breaks sharply at the summit, whereas the top would almost certainly be smoothed off after the manner of most biological functions. Nevertheless the figure undoubtedly exhibits the general form of this contour.

At higher levels of saturation the contour would break up into two closed curves, one encircling the peak of saturation in the reds and the other the peak in the violets. This is a level of saturation higher than any yellow can ever attain.

Some of these contours for saturation ought to be determined directly. The plotting of them would increase our knowledge of the chromatic relationships. The use of them would emphasize the fact that the three attributes of color can be understood as functions of only two variables in the stimulus, for this note succeeds in discussing all three of the photopic attributes without any reference to complexity, which is conventionally held to be "the" third dimension of the stimulus. In fact there is no reason why an organism could not become aware of hue, brilliance and saturation as three distinct entities even though his retinas had never been exposed to anything but homogeneous lights.

Harvard University EDWIN G. BORING

ON THE METHOD OF BISECTION AND ITS RELATION TO A LOUDNESS SCALE

The construction of a scale for the measurement of a subjective magnitude requires the adoption of some basic method for defining units on the scale. One such method has included the determination of differential thresholds, which were then assumed to be units of magnitude. Two other methods have recently proved more fruitful. One of these may be called the method of *fractionation.* It requires an observer to adjust a variable stimulus until it appears to have a certain fraction of the subjective magnitude of a standard stimulus, for instance, half the loudness, half the brightness, half the pitch. The other method, an older one, is that of *bisection* or equal sense distances. In this method the *O* is asked to adjust a variable stimulus until it is mid-way between two limiting stimuli. It is closely analogous to the method of fractionation with the exception that a stimulus occurs at both ends of the magnitude bisected.

Data recently obtained by the fractionation of loudnesses have shown sufficiently good agreement to warrant the construction of a loudness scale (Churcher).[1] This scale flatly disagrees with the scale obtained on the basis of the assumption that all differential thresholds are subjectively equal (Stevens).[2] What, then, is the relation of this loudness scale to data obtained from the method of bisection? The loudness scale obtained by fractionations would clearly be more satisfactory if it could also be derived from bisections.

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[1] B. G. Churcher, A loudness scale for industrial noise measurements, *J. Acoust. Soc. Amer.*, 6, 1935, 216-226.

[2] S. S. Stevens, A scale for the measurement of a psychological magnitude: Loudness, *Psychol. Rev.*, 43, 1936, 405-416.

Before the method of bisection is used extensively for making such a check, certain objections to it, on the grounds of internal evidence, must be answered. Gage has recently shown that results of bisections of loudness-intervals may lack internal consistency.[3] His *Os* first bisected a distance to obtain the half-way point. They then in turn bisected the upper half to obtain the three-quarter point and the lower half to obtain the one-quarter point. Finally they bisected the distance between the one-quarter point and the three-quarter point to yield a second half-way point. This second half-way point should be the same as the original half-way point. In fact, however, Gage found that the final bisection was consistently higher (louder) than the initial bisection. He concluded, therefore, that the method is invalid, and went so far as to question the possibility of measuring subjective magnitudes at all. Before admitting the impossibility of constructing a scale for sensory magnitude, however, the experimental findings need to be examined more closely.

We have repeated Gage's procedure with changes in two conditions which might

TABLE I

SUCCESSIVE BISECTIONS OF LOUDNESS

Initial stimuli: R_1 = 80 db; R_5 = 100 db loudness level

	Volkmann	Newman
R_3 (bisection of R_1 and R_5)	89.45 db	90.03 db
R_2 (bisection of R_1 and R_3)	84.80	84.90
R_4 (bisection of R_3 and R_5)	95.05	95.50
R'_3 (bisection of R_2 and R_4)	89.63	90.39

have been responsible for his results. Instead of using Gage's very low loudness level, we used a relatively high one, and instead of an ascending order of presentation alone, we used alternately an ascending and a descending order. Pure tones of 1000 cycles were presented to *O* through a pair of loud-speaker units used as ear phones. An automatic timing-relay presented the three loudnesses in succession; each loudness lasted 2 sec. with no pause between successive loudnesses. Ascending and descending series followed each other at intervals of 2 sec. The *O* was able to adjust the second tone of the series of three until it appeared equidistant in loudness between the first tone and the third. He first bisected the distance between a loudness level of 80 db (R_1) and a loudness level of 100 db (R_5). The result of this bisection was called R_3. He found in turn R_2 by bisecting R_1 and R_3, R_4 by bisecting R_3 and R_5, and then made the final bisection of R_2 and R_4 to find R'_3. The results for the two *Os* are presented in Table I. Each of the values in the table is the geometric mean of 10 settings.

An inspection of this table shows that the discrepancies between R_3 and R'_3 are small for both *Os*. In the one case R'_3 was 89.63 db as against an initial 89.45 db, in the other case 90.39 db as against 90.03 db. Whereas Gage found differences of 5 and 6 db working over a range of 40 db, we find differences of only 0.18 and 0.36 db for a range of 20 db. Actually, however, we were bisecting an interval, measured in terms of a subjective loudness-scale, which was several times as long as that used by Gage. It is not altogether clear what variable or variables might account for differences between the two experiments. Our results indicate, however, that the method of bisection *can* yield consistent results under certain conditions.

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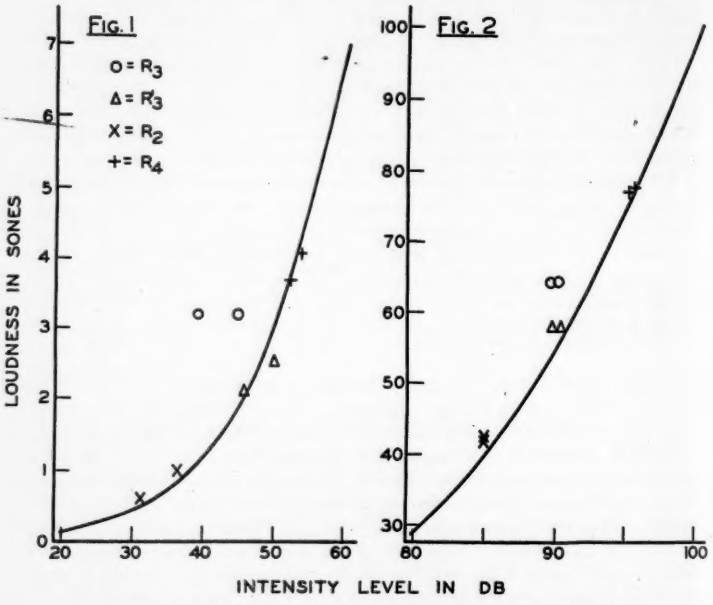
[3] F. H. Gage, The measurability of auditory sensations, *Proc. Roy. Soc.* 116B, 1934, 103-119.

While a series of bisections, such as Gage used, constitutes a valuable check on the method of bisection and may reveal constant errors in the method, it also tends to magnify both constant and variable errors which are present in the individual bisections. Thus, for example, if *each* bisection deviates from the true mid-point by plus 10% of the interval bisected, the successive bisections, on a scale of 100,



FIGS. 1 AND 2. RELATION OF BISECTIONS TO THE LOUDNESS SCALE

The solid curves represent sections of the function relating the magnitude of subjective loudness, in sones, to the intensity of the stimulus (at 1000 cycles). The pairs of points represent the bisections of loudness intervals—one point for each *O*. Fig. 1 shows Gage's results and Fig. 2 our results. Each point represents the bisection of the interval between two values, both of which were adjusted to fall on the loudness function.

would be 60 for R_3, 84 for R_4, 36 for R_2, and 64.8 for R'_3. The 10-point deviation of R_3 from the true mid-point has been increased to a deviation of 14.8 points for R'_3. Probably all metric procedures are subject to both constant and variable errors, and it is the task of experimentation to discover these errors and to eliminate them as far as possible. It would seem reasonable, therefore, when there is a lack of consistency in the results, to look for constant errors rather than to question the validity of the entire procedure.

Do these results secured by the method of bisection, fragmentary as they are, tend to confirm the loudness function determined by the method of fractionation?

Figs. 1 and 2 present results obtained by the two methods. In each figure the solid line represents the loudness function as constructed by Churcher. The results of Gage's bisections are plotted as points in Fig. 1, and our results in Fig. 2. In order to avoid cumulative errors, the end-points for each bisection in turn have been fitted to the loudness function. Certain of the points fall well off the curve; these are typically the bisections of long intervals, values of R_8. The majority of the points, however, coincide rather closely with the loudness function. In Gage's results there are actually small deviations in both directions from the loudness function. These results suggest that, if constant errors could be minimized, the method of bisection would yield results essentially equivalent to those of the method of fractionation.

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THE LAG OF PUBLICATION IN JOURNALS OF PSYCHOLOGY

The present delay in the publication of psychological articles is a matter of grave concern within the profession.[1] There is the general impression that the delay is steadily increasing. The practice of some journals of printing extra material immediately when the additional cost of manufacture is paid by the author has not materially relieved the situation. The new *Journal of Psychology* takes only such paid articles for immediate publication, but a method which increases the cost of research to the individual is not desirable. For this reason the *Journal of Experimental Psychology* and this JOURNAL (*vide infra*) both announce that they will hereafter publish two volumes a year instead of one.

In the meantime it seems desirable to examine the situation genetically in order to assess present conditions accurately. Table I shows the history of these lags for the samples and the journals listed. In it the amount of lag is taken as the displacement of the month of publication from the month in which the article was received or accepted, as that date is printed in the journal. The *apparent lag* accepts the date printed on the cover of the journal as the date of publication. The *actual lag* takes account of the date of receipt of the journal as stamped upon it in the Harvard College Library. For instance, the *Journal of General Psychology* for July, 1936, showed an apparent lag of 14 months, but the actual lag is 17 months since this number was delivered in October. (Date stamps were lacking for the two journals which are taken only by the Departmental library, but not by the Harvard College Library.)

Certain differences in editorial policy appear. The *British Journal of Psychology* dates all articles but apparently determines the makeup of a number with regard to editorial values other than date. The *Journal of Comparative Psychology* does not date articles and could not therefore be included in this survey. It, the *Journal of Experimental Psychology* and this JOURNAL all have been accepting articles for immediate publication as extra material, which increases the size of the volume since the author pays the cost of publication. Such articles are readily determined

[1] See J. E. Anderson, Report of round table on "Time lag in the publication of psychological research," *Psychol. Bull.*, 32, 1935, 855-858; also Publication lag in the publications of the American Psychological Association, Inc., *ibid.*, 33, 1936, 581 f.

by their recent dates in the two latter journals, and were ignored in making up the averages of the table. The three last journals in the table, which have been published by the Clark University Press, do not date all articles. Inspection makes it appear that some articles from Clark University, from its recent graduates and from a few others are printed without date.[2] The editor of these journals has not replied to a request for a statement of his policy. There are some inadvertences about omitted dates in other journals, but the editors explain them all as oversights which occurred mostly at the time of transition from the practice of no dating to the practice of dating. Of course, all undated articles have been ignored in compiling the table. With the exception of the *British Journal of Psychology*, and of the articles where the author pays for publication in the *Journal of Experimental Psychology* and this JOURNAL, and of the undated articles in the *Journal of General Psychology*, the *Journal of Genetic Psychology* and the *Journal of Social Psychology*, it appears that all the journals listed try to follow the practice of first-come-first-serve. A minor exception appears when, in the process of make-up, it happens that a later short article can find space in a number when there is no room left for an earlier long article.

TABLE I

LAG OF PUBLICATION IN JOURNALS OF PSYCHOLOGY

	Amer. J. Psychol.	Psychol. Rev.	J. Exper. Psychol.	J. Abn. & Soc. Psychol.	Brit. J. Psychol.	J. General Psychol.	J. Genetic Psychol.	J. Soc. Psychol.
Apparent lag:								
1928 (spring)	12	5	7	—	9	6	4	—
1930 (spring)	8	6	10	—	9	18	14	15
1932 (spring)	14	8	14	—	5	7	12	14
1934 (spring)	20	7	15	13	10	11	11	13
1936 (spring)	18	9	15	17	11	14	7	17
1936 (fall)	18	8	12	15	11	14	—	18
Actual lag:								
1928 (spring)	12	5	7	—	10	8	—	—
1930 (spring)	8	6	10	—	11	19	—	17
1932 (spring)	13	8	14	—	5	8	—	15
1934 (spring)	21	7	15	—	10	14	—	13
1936 (spring)	18	9	15	—	11	14	—	18
1936 (fall)	18	8	12	—	11	17	—	20
Prediction 1936 (fall)	9	6	11*	18	—	—	—	—

* Now reduced further by the doubling of the number of issues per year.

The table shows great irregularity. There are probably seasonal differences which do not appear in it. A new journal starts with little lag but is soon back with the others. One new journal may temporarily remove pressure from another. There are fortuitous variations. The pressure is against all kinds of psychology, except

[2] For instance, in vols. 10 and 11 of the *Journal of General Psychology* (1934), 10 out of 43 articles are undated, and, in vols. 12 and 13 (1935), 15 out of 43 articles are undated. Of these 25 undated articles, 13 are from Clark or from an author with a recent Clark Ph.D.

that it is less perhaps for theoretical psychology (*Psychological Review*).

Editors are beginning to find, however, that they are not quite helpless in a situation that does not right itself. The last line of the table shows the *prediction*, that is to say, the approximate lag that could be predicted for manuscripts accepted in the fall of 1936 before the *Journal of Experimental Psychology* decided to increase its size. There had been in a short time radical improvement in the *Journal of Experimental Psychology*, the *Psychological Review*, and this JOURNAL, due, it would seem from correspondence with the editors, to stricter editorial standards as to quality and length. There is no evidence that the lag is yet diminishing for the *Journal of Abnormal and Social Psychology*, the *British Journal of Psychology*, the *Journal of General Psychology*, or the *Journal of Social Psychology*.

The conclusion is that, as Fernberger averred,[3] a rigorous editorial policy can hold the lag to whatever amount of latency is desirable. A steadily increasing lag which approaches no limit is intolerable. If equilibrium is to be maintained editorially, there will, after a short period of readjustment, be no loss of material if it is held at six or nine months instead of at fifteen or twenty months, and the gain that prompt publication gives in the stimulation of research will be tremendous.

Harvard University EDWIN G. BORING

EDITORIAL NOTE: THE LAG IN PUBLICATION

In view of the increasingly serious lag in psychological publication—a problem which Professor Boring brings to a head in this number of the JOURNAL (*supra* 137—the editors of the JOURNAL announce that they will publish two volumes in 1937[1] and annually thereafter so long as the increase appears necessary for the prompt publication of all good, tersely written papers.

When the lag in publication began, in 1927, to exceed a year, the JOURNAL, in order to protect the priority of its contributors, adopted the policy of publishing the date of acceptance of its articles.[2] This practice was put into effect in 1928 and it has since been adopted by most of the psychological periodicals.

Dating, however, brought with it the problem of place-crediting. The practice of the JOURNAL up to the present time has been to show on the place-line after the author's name, the institution at which the research was done, even though the author by the time of publication had moved to another place. Thus the notation has assigned proper institutional credit, but has been inadequate as an address or as a record of the institutional connection of the author at the time of publication. We have concluded that these two latter advantages outweigh the former. Therefore, beginning with the present number, the JOURNAL will give in its place-line the institutional affiliation of the author as of the *date of publication*. A different affiliation at the time of acceptance of the article will be shown in the first foot-note.

K. M. D.

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[3] *Loc. cit., Psychol. Bull.*, 33, 1936, 582.

[1] Volume 49 will be published at the regular dates of issue and Volume 50 in November, 1937, in celebration of the Golden Jubilee of the JOURNAL.

[2] Cf. the editorial note, The dating and publishing of manuscripts, this JOURNAL, 40, 1928, 172.

The Forty-Fourth Annual Meeting of the American Psychological Association

The Forty-Fourth Annual Meeting of the American Psychological Association was held at Dartmouth College, Hanover, New Hampshire, on Wednesday, Thursday, Friday, and Saturday, September 2, 3, 4, and 5, 1936. A new attendance record was established, the total being 851 persons, classified as follows: 191 Members, 2 newly elected Members, 312 Associate Members, 51 newly elected Associates, and 295 persons not affiliated with the Association.

At the Business Meeting, 6 Members were elected, 37 Associates were transferred to the status of Member, and 199 Associate Members were elected. The invitation of the University of Minnesota to hold the 45th Annual Meeting at Minneapolis, Minnesota, was accepted and the following dates were fixed: Wednesday through Saturday, September 1, 2, 3, 4, 1937. Richard M. Elliott was appointed a member of the Executive Committee for 1936-1937, and Florence L. Goodenough, Herbert Woodrow, and the Secretary were elected as the Program Committee for 1937. Newly elected officers are: President, Edward Chace Tolman; Treasurer, Willard L. Valentine; Directors, Gordon W. Allport and John A. McGeoch; Division of Anthropology and Psychology, N.R.C., Henry E. Garrett, Arnold Gesell, and Calvin P. Stone; S.S.R.C., Mark A. May; A.A.A.S., Walter R. Miles and Christian A. Ruckmick. Donald K. Adams was elected as a member of the Committee on Precautions in Animal Experimentation. The invitation from the National Occupational Conference to have the Association appoint a representative was accepted and Percival M. Symonds was appointed for the three year term, 1936-1939. William A. McCall was appointed as a representative on the Sectional Committee on Standards for Graphic Presentation of the American Standards Association and Albert T. Poffenberger was appointed as a representative on the New York Management Council Board of Directors.

The Association voted to continue affiliation with the Inter-Society Color Council for the year 1936-1937, and re-appointed the present voting delegates and additional delegates. The Association voted to continue the Committee on Psychology of the National Advisory Council on Radio in Education with Walter V. Bingham as Chairman, Goodwin B. Watson, Hadley Cantril, and Gordon W. Allport as members. The Committee on Motion Pictures and Sound Recording Devices in Instruction of Psychology was continued with its membership unchanged except that Edgar A. Doll is to assume the Chairmanship. The Committee on the Experimental Study of Suggestion reported its belief that codification of rules of procedure is inadvisable at this time and was discontinued. The Secretary's report on Affiliation of National, Regional, and Local Organizations including the proposed changes in the By-Laws was adopted. The status of existing national, regional, and local branches and affiliates is unchanged. Petitions for affiliation from the Association of Consulting Psychologists and from the Society for the Psychological Study of Social Issues were received, discussed, and filed for action at the 45th Annual Meeting of the Association. The Committee to Study Problems Connected with the Teaching of Psychology in High Schools and Junior Colleges was continued and was authorized to conduct a nation-wide canvass of the situation. The Committee on the Social Utilization of Unemployed Psychologists reported the results of its survey of 214 cases and concluded that the unemployment of psychologists is only in part a product of the

depression, the other factors being the saturation point in the academic field and the failure of departments to select graduate students on the basis of personality as well as intellect. On the recommendation of the Committee, it was discontinued. The Committee on Psychology and the Public Service reported progress and was continued. In response to a request from the Psychologists' League that the Association endorse a W.P.A. project drawn up by the League, the Association voted to appoint two representatives to collaborate with the Psychologists' League and the Association of Consulting Psychologists to consider the feasibility of working out ways and means of increasing the opportunities for psychological service in education, government, social service, and business and industry.

The report of the Committee on the Relation of the Association to its Publications including the proposed changes in the By-Laws was adopted. Briefly, the management of the publications is placed in the hands of the Council of Directors, terms of office for editors are established, the duties of the Editorial Board and the Business Manager are defined, and an Electoral Board is established for the election of new editors. The progress report of the Committee on Securing Financial Support for the Continuation of *Psychological Abstracts* was accepted and the Committee continued. A Committee to Study the Problem of Costs of Continuing the *Psychological Abstracts* for the Next Ten Years was also appointed. A progress report was made by the Committee to Study the Costs of Conducting the Affairs of the American Psychological Association as now Operated and to Make Estimates of Probable Costs of other Modes of Organization and the Committee was continued. A recommendation from the Board of Editors that the name and scope of the *Journal of Abnormal and Social Psychology* should be changed to the *Journal of Abnormal Psychology* was laid on the table. A Committee consisting of John E. Anderson, Chairman, and E. S. Robinson and Leonard Carmichael as members was appointed to consider the desirability of formulating by-laws for establishing an honorary membership classification. The Association also authorized a mail ballot of the entire membership to obtain an expression of opinion regarding program making policies. The Association also voted to authorize the Council of Directors to schedule meetings for two or more years in advance. Institutions planning to invite the Association in the near future should communicate with the Secretary.

Twenty-three formal sessions were scheduled and 143 papers were presented. For the first time, a formal session on political psychology was scheduled. Another significant development is the relatively large number of quantitative studies of psychotic patients reported in two sessions on abnormal psychology. No session on clinical psychology was scheduled. Papers on animal psychology and physiological psychology predominated again. Nevertheless, continued research activity is apparent in such a broad array of topics as sensation and perception, vocational psychology, child psychology, learning, memory, work and fatigue, educational psychology, emotion and motivation, and social psychology.

On Wednesday evening, twelve research and instructional films were exhibited. Three round tables on Clinical Psychology were held. The Section on Clinical Psychology elected Dr. Gertrude Hildreth as Chairman for 1936-1937 and Dr. Edgar A. Doll as member of the Executive Committee for 1935-1938. A total of five Members and Associates was admitted to the Section. The Psychometric Society elected Dr. E. L. Thorndike, President for 1936-1937, Dr. Jack W. Dunlap, Secretary and Dr. Albert K. Kurtz, Treasurer. One of the formal scheduled sessions was

devoted to papers on psychometrics. Additional round tables and panel discussions were held as follows: the Subject Matter of Social Psychology, Qualifications of Industrial Psychologists, the Subject Matter of the Course on Educational Psychology, Methodology in Learning, and Psychological Theory. This last named Round Table was devoted to a discussion of Recent Advances in the Conception of the Conditioned Response at which time Elmer K. Culler presented a forty minute paper as a basis for the ensuing discussion.

On Friday evening, the Presidential Address, "Mind, Mechanism, and Adaptive Behavior," was given by Clark L. Hull. President Hull summarizes the address as follows: "The controversy between the physical and the psychic views as to the nature of adaptive behavior is based on a disagreement in theory rather than in fact. The proper procedure for the settlement of such a controversy is for the protagonists each to develop, if he can, a strictly scientific theoretical system (as contrasted with philosophical speculation) and then examine the postulates of the successful system to determine their essential nature. Since none of the parties to the controversy has fulfilled this most elementary requirement for reaching a settlement of the dispute, there was presented for purposes of illustrating the methodology to be employed a miniature theoretical system consisting of eighteen definitions, six postulates, and thirteen theorems, the latter derived by the geometrical method. An examination of the six postulates of this system suggests that adaptive behavior is probably at bottom physical rather than psychic."

Following the Presidential Address, Dartmouth College entertained members and guests in College Hall where a reception was held and light refreshments were served.

Uniformly excellent facilities were provided for the meeting. Professor Charles Leonard Stone, his colleagues, and the administrative officers of the College proved themselves to be ideal hosts.

University of Minnesota DONALD G. PATERSON

THE ELEVENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Eleventh International Congress of Psychology which was to have been held in Madrid, Spain, on September 5-12, 1936, will be held in Paris, France, on July 25-31, 1937.

Professor Pierre Janet is the Honorary President of Congress; Professor Henri Piéron is the Chairman of the Organization Committee; and Professor I. Meyerson is the General Secretary.

The head-quarters of the Congress will be at the Psychological Laboratory of the Sorbonne, Paris. The plans for the program will remain in general those outlined for the Congress that was to have been held in Madrid.

K. M. D.

THE CHINESE JOURNAL OF PSYCHOLOGY

The first number of *The Chinese Journal of Psychology,* dated September, 1936, has been received. It is edited by Professor C. W. Luh of Yenching University and Professors K. H. Sun and Siegen K. Chou of Tsing Hua University; and is published by the Department of Psychology of Yenching University, Peiping. The new journal, "founded in the interest of all branches of scientific psychology, espe-

cially laboratory psychology," is to be issued quarterly in September, December, March, and June.

The first three studies in the September number are experimental and are published in English with abstracts in Chinese. They total 53 of the number's 90 pages, and are: K. H. Sun and F. T. Dun, Note on geotropism in the dark-adapted planaria gonocephala; M. Chang, Neural mechanism of monocular vision: I. Disturbance of monocular pattern discrimination in the albino rat after destruction of the cerebral visual area; C. T. Lin, The factors of similarity and identity in retroactive inhibition. These articles are followed by an analysis and exposition of Thorndike's Psychology of Learning (J. C. Tsao); a review of Koffka's Principles of Gestalt Psychology (C. W. Luh); a section devoted to Notes and News; and a Postscript—all printed in Chinese.

This JOURNAL welcomes the new *Journal* into the psychological fold.

K. M. D.

BOOK REVIEWS

Elements of Psychology. By KNIGHT DUNLAP. St. Louis, Mo., C. V. Mosby Co., 1936. Pp. 499.

Based upon, but not technically a revision of the author's earlier *Elements of Scientific Psychology,* the present volume represents a conception of the purpose of introductory texts clearly divergent from contemporary trends. It attempts first of all to provide a clearly defined set of concepts, together with the author's own critical interpretation, rather than to prepare the student to judge the evidence for himself. The author specifically rejects, however, any intention of providing "easy steps for little feet," in favor of a book "which shall be worth serious study and which should repay the labor of which the average undergraduate is capable." He has "sought for *clarity* and *precision,* rather than simplicity," (italics mine) a point which must be borne in mind during later criticisms. Contrary to a growing practice among other texts, this book deliberately omits summaries of specific experiments, declaring this to be the task of the instructor, when necessary. Stating also that "experimental work for elementary students, especially for large groups, degenerates almost of necessity into busy work of a trivial nature, which not only wastes the student's time but also gives him either a contempt for experimental psychology or else a fallacious belief that he has an important grasp on it," the author recommends demonstrations as a substitute. Evidently the book is not intended to provide easy steps for young instructors, either, if they are to be solely responsible for introducing the student to experimental methods of evaluating presentations.

Somewhat shorter than other texts, both as to number of pages and chapters, the major structure of the book seems to be primarily an elaborate classification and definition of "the elements" or those basic concepts of psychology which the author finds necessary for the understanding of more complex practical activities. Accompanying this is a running commentary of the author's evaluation of various points, critically challenging, but difficult for the reader to evaluate himself because of the absence of experimental details which might support or deny the interpretation. Particularly in the discussion of the sense fields, but to a lesser extent throughout the book, numerous rare and new terms are introduced for the purpose of clarifying and systematizing the treatment of conscious phenomena. The "Table of the Senses," (pp. 52-53) for instance, giving for each sense field the common and technical terms for the sensory process, its privative, mechanism, and stimulus will be variously hailed as a masterpiece of philological research by a few specialists in those fields, and, I fear, either as a source of confusion or a thing to be skipped over by the students.

The laudable attempt to clear up misunderstandings in both popular and scientific usage of scientific terms is helpful in many instances, but in others introduces rare or new terms which even in the considerable period since the author's earlier elementary texts have not been taken up by other writers and are therefore likely to be of little further use to the student.

Although the chapter headings and their order are principally those of traditional "pure" psychology, the treatment given topics usually represents the author's own very critical recasting of interpretations. Implicit motor responses, rather than

neural activities alone, are suggested as a probable physiological basis of conscious processes, not a new point, but one which few other text writers have given adequate consideration.

Students of such special topics as instinct, imagery, and emotions will find a needed sifting of concepts but are likely to disagree in a number of instances with particular interpretations drawn. On the topic of learning one is surprised to find the work of Pavloff on conditioned reflexes in animals and the work of later investigators on conditioned responses in man dismissed with only brief consideration and with slight importance attached to them. Other theories of learning receive even less consideration, the major emphasis being upon a series of rather practical suggestions on necessary factors in learning from the standpoint of immediate applications rather than as a basis for understanding other materials on this subject. The importance of learning is reëmphasized in succeeding chapters.

The chapter on "Individual Differences" attempts to cover too large a field in too short a space. Intelligence testing and theories receive an unusually critical treatment which will probably leave the student duly skeptical of the field, but will also leave him without many facts on this practically important topic. Advocates of intelligence testing will probably find this treatment very irritating, both as to criticisms of current theories and practices and the neglect of important evidence in their support. The topic of personality receives scant consideration, the general concept being presented as a passing interest, any important aspects of which are better treated under standard topics of psychology. The final chapter on "Maladjustment and Readjustment" presents definitions and interpretations of concepts which may be helpful to the student in understanding the topic during the course but in some cases are so unusual as to cause probable confusion in later readings of other authors of abnormal psychology.

In summary, this text represents an attempt to give clarity and precision, rather than simplicity, to an extensive group of important concepts, with elaborations of certain less well known topics such as desires. As a part of this classification numerous current weaknesses are exposed, and the hypothetical nature of other beliefs is pointed out. The newer applied fields of psychology are given only passing mention as compared to the treatment found in other elementary texts. The terminological distinctions and the points of view presented offer a needed stimulus toward critical consideration of many topics in psychology, but in many cases seem more appropriate to advanced rather than to introductory psychology. It remains for the users of the text to determine whether the absence of experimental summaries avoids confusion to the elementary student, simply omits this part of his education, or results in an adequate presentation of this aspect by instructors. For advanced students the provocative style of the book provides an excellent starting point for criticism of current viewpoints in many fields.

University of Southern California ROBERT H. SEASHORE

Law and the Lawyers. By EDWARD S. ROBINSON. New York, Macmillan Co., 1935. Pp. xi, 348.

This book is addressed to lawyers or at least to those lawyers who have some interest in jurisprudence. They are urged to give up their conservative, formalistic and rationalistic point of view and instead to adopt a realistic philosophy of the law. The plea is made, however, by a psychologist, and the particular brand of

realism which is proposed is mainly psychological. There is, therefore, much in the book to interest the psychologist.

If it is the business of the law to guide and to control the behavior of the individual whenever it seriously affects other individuals, the lawyer should, in the author's opinion, know all that cultural anthropology and psychology can teach him about human behavior. Furthermore, he should be a technologist in social science much as the physician and mechanical engineer are technologists in the biological and physical sciences. Since social science is regarded as a natural science, the lawyer as legal technologist would adopt a scientific attitude—a fact-finding attitude toward his problems. In so doing he will discover that many of the psychological theories which now furnish the basis of much legal thinking are either outmoded or utterly mistaken. He will also find that what ought to be done by way of legal control will frequently be determined, not as it now is by the products of habituated thinking, but by the social and psychological facts. In the light of these facts moral ideas may be expected to be re-formed, and what is best for both society and the individual may be envisaged from a scientific point of view rather than from that of a vague common-sense morality.

What psychology, then, should the jurist know? The author warns against the adoption of the tenets of any one school like Behaviorism or Freudianism, for a single school is ephemeral and almost certain to ignore some facts that the lawyer needs. The jurist should therefore be able to evaluate all schools; that is to say he should "become his own competent critic on matters of psychological theory." He should also know the many complex and variable factors that may dominate the behavior of the individual in any particular situation. Not that this knowledge will furnish the lawyer with ready-made judgments but because it "will provide guides to better judgments." Finally he should know enough psychology to be on guard against the dangers that lurk in the human tendency to personification, and in the tendency to think in terms of faculties, psychological forces, mental substances, the unconscious, conflicts, rationalization, and even of nebulous concepts of the individual and of society.

Not content with the mere plea for a social-psychological attitude toward the law, and the indication of the kind of training the lawyer would need, the author undertakes also to show what would result if the naturalistic attitude were taken toward such problems as judicial deliberation, judicial problems, judicial formulas and legal rules. The materials are found in judicial utterances, particularly in the reports of the appellate courts. These are regarded as facts of behavior, and the analysis of them is carried far enough to show that much judicial thinking is clouded with vague psychological concepts, and that it is guided not solely, as is supposed, by deductive and analogical methods but also by common-sense psychological, ethical and economic notions of which the judge is often quite unaware. There is also an excellent chapter showing what the jurist might gain from a psychological study of social adjustments.

The book is, or should be, stimulating both to the jurist and to the psychologist. There are times, it is true, when the author is not entirely convincing, times when he seems to be too much concerned with the law in theory and not enough with the law in practice, times also when one wonders if he is not laying too great a psychological burden upon the legal profession and if he has sufficiently realized that psychology cannot solve all the problems that will arise for the legal tech-

nologist. When, for example, the law, as an instrument of social control, should or should not be applied is a problem of social philosophy in the solution of which psychology can do no more than give aid. There is not the least doubt, however, but that if the proposed attitude could in a few decades become a common property of all lawyers, the legal profession would be revolutionized. The practicing lawyer, whose outlook does not now extend beyond the mere giving of good legal advice and the winning of cases, would become a constructive force in the community, and the law would become less dominated by legal tradition and more flexibly adapted to aid in the solution of social problems as they arise.

The psychologist will hope with some misgiving that his facts are concrete and definite enough to satisfy the demands which the lawyer would lay upon him, but he will welcome not only the application of his knowledge in a new field but also the new problems which may be expected to arise. Psychology may indeed already be grateful to the author for opening up hitherto unexplored avenues for investigation.

Cornell University H. P. WELD

The Future of Marriage in Western Civilisation. By EDWARD WESTERMARCK. New York, Macmillian Co.. 1936, Pp. xiv, 281.

This volume is of special interest because it represents the present-day position of the deservedly famous author of *The History of Human Marriage* in three volumes, first published in 1891 and re-written in 1905. While the emphasis in the present volume is upon the future of marriage, this specific problem is obviously bound up with the larger one regarding the nature of the marriage institution as revealed by the changes which it has undergone in the past. We are not surprised, therefore, to find the author falling back repeatedly upon his earlier historical treatment in seeking to determine the future trend of events. He also brings to bear upon the problem the more recent findings and opinions of anthropologists and sociologists.

Professor Westermarck criticizes his former definition of marriage as "a more or less durable connection between male and female, lasting beyond the mere act of propagation till after the birth of the offspring." He now regards this concept as being too broad—since a man and woman may live together and have children without being married. Marriage is a social or cultural institution which involves the sanction of custom or of law. Marriage is a cultural trait which grew out of the family life of primitive man and this, in turn, rested upon the earlier primate family. The author believes, in common with most modern anthropologists, that the small family or harem had evolved from the "primitive horde" long before man came upon the scene. The presence of this social unit among the higher primates and man is to be explained in terms of the sexual, maternal, and paternal instincts. Marriage is closely related to this basic complex, but includes as well the channeling of a primitive habit of life along institutional lines.

Although the essential elements of normal marriage are sexual, procreative, and the husband-wife relationship, one or another of these may be absent or distorted without putting an end to the marriage institution. Those who hold that this institution will ultimately be discarded in favor of "free-love" in some form usually appeal to the increasing divorce rate, the increase in extra-marital relations, the lessening desire for parenthood, and the like. It should be noted, however, that a large percentage of divorcees eventually re-marry and thus stay within the system.

Moreover, promiscuous sex relationships seldom provide the security and prestige that has come to be associated with the family menage. This may account in part for the fact that an extra-marital sex life is often added to, rather than substituted for, the marriage relation. In any case, it is unsafe to predict the future on the basis of present trends in our social life, since we cannot be certain that these will continue indefinitely. The author believes that the demands for the family type of social unit are deep-seated enough to survive the changes through which the marriage pattern is now passing. This is merely a conviction based upon the weight of evidence now at hand.

The volume is well-documented but is written in an easy style which renders it suitable for the educated layman.

Columbia University C. J. WARDEN

Comparative Psychology : I. Principles and Methods. By CARL J. WARDEN, THOMAS N. JENKINS, and LUCIEN H. WARNER. New York, Ronald Press Co., 1935. Pp. x, 506.

This book is the first of a proposed three-volume comprehensive work on comparative psychology. As the subtitle indicates, this volume is devoted to a consideration of fundamental points of view and methods in comparative psychology. The work is divided into seven large chapters or parts, which deal roughly with the following aspects of the subject: (1) history; (2) the standpoints of teleology, vitalism, and natural science in the study of behavior; (3) evolution and behavior; (4) the classification of behavior; (5) methods of experimental study of behavior in relation to the receptors; (6) methods of studying behavior in relation to motives, modifiability, intelligence; (7) comparative morphology and physiology of organisms in relation to behavior. The first six of these sections were written by Dr. Warden, the last chapter by Dr. Jenkins. Over two hundred references are appended and an adequate author index and subject index provided. The volume is generously illustrated.

This work, while intended as an advanced student manual, is much more than a mere textbook. It is a real step forward in the history of scientific psychology. Here possibly more adequately than before, comparative psychology is shown in its proper place in the structure of the biological sciences. Throughout the book the scientific approach to the large field of the behavior of living organisms has been rigorously maintained. In many places the authors have given the weight of their authority to the support of sound procedures, some of which have not always been popular in comparative psychology. As an example of such a statement the reviewer cannot refrain from quoting the following sentence: "The sampling of a number of functions, by the use of short-cut methods, cannot take the place of the systematic analysis of receptive and reactive capacities as the proper basis for evaluating intelligence level."

In dealing with the material of the last section of the book, in which the knowledge of the receptors, nervous system, and effectors of the whole animal series is summarized, Dr. Jenkins was faced with a task of condensation even greater than that required in the previous chapters, and while universal agreement in such a selection is, of course, not to be expected, the presentation given is valuable.

The reviewer's special interest in comparative psychology makes him wish that in the section on the biological foundations of behavior more factual material had been given concerning the development of behavior in ontogeny.

American psychologists who are interested in animal behavior will await with interest the companion volumes to the one here reviewed. The following volumes will have to be excellent if they are to maintain the standard set by the present book.

University of Rochester LEONARD CARMICHAEL

Comparative Psychology: III. Vertebrates. By CARL J. WARDEN, THOMAS N. JENKINS, and LUCIEN H. WARNER. New York, Ronald Press Co., 1936. Pp. x, 560.

The volume under review is the second to be issued in a set of three volumes presenting what the authors justifiably term a complete treatise on comparative psychology. The present volume, however, is Volume III of the series. It was prepared by Dr. Lucien H. Warner.

A taxonomic approach to the field has been employed in outlining the really vast amount of material on vertebrate behavior which is the subject matter of the present volume. Six chapters, one each on *Pisces, Amphibia, Reptilia, Aves, Mammalia (Except Primates),* and *Primates,* have been developed. In these chapters, receptor functions are first treated. Next, reactive capacities, including both responses described in terms of effector acts such as specific reflexes and more complex forms of behavior described in terms of ends such as migration and tool-making, are presented. Finally, facts concerning the modifiability of behavior in the phylum under consideration are presented.

It is interesting to contrast this method of outlining the material on comparative psychology with that which employs psychological or physiological functions as the basis of organization, as is the case in a number of other books on comparative psychology and notably in *The Animal Mind* by M. F. Washburn. In the reviewer's opinion, the "taxonomic" as contrasted with the "problem" classification has the advantage that the total behavior of the organism is probably more readily presented under the plan as used in the present volume. This very fact, however, leads the authors, for better or for worse according to one's bias in comparative psychology, to include much material at the "natural history" level which, although interesting, is not amenable to any scientific generalization. Thus for example, the full nature of the quantitative study of visual function in the phyla here considered is not presented but rather a description is given of experimental and observational studies of behavior of the total organism which may be thought of as determined by visual stimulation. It is important in considering the treatment of these facts in this volume to consider them in connection with the principles presented in the first volume.

The reviewer cannot refrain from again expressing his enthusiasm, as he did in the review previously published of the first volume, that the authors of this work have been able to bring together in convenient form so large a body of material relevant to the significant field of comparative psychology. The advanced student and the research worker will find these volumes of constant value.

At the end of the present volume, there is a list of journals to which reference has been made in the bibliography. The bibliography itself is presented by chapters. In all, there are 2030 titles listed. An author and subject index is also appended. A slight error in pagination has occurred in the present edition, but this will no doubt be corrected in subsequent printings.

University of Rochester LEONARD CARMICHAEL

Zum Problem der aussersinnlichen Wahrnehmung: Ein Beitrag zur Untersuchung des "räumlichen Hellsehens" mit Laboratoriumsmethoden. By HANS BENDER. Leipzig, J. A. Barth, 1936. Pp. xi, 116.

This volume presents an experimental investigation of "supersensory perception" or clairvoyance, which was carried on at the Psychological Institute of the University of Bonn. There is an introductory note by E. R. Jaensch, in which he commends Bender's work, explaining that it is only by such careful and unprejudiced scientific investigations that the misconceptions regarding parapsychic phenomena can be dispelled. Immediately preceding the text is a list of technical terms with definitions. One entire chapter is devoted to a historical survey. There is a good bibliography.

The experimental work consists of four series and an unnumbered "negative" series. One person, Frl. D., acted as *O* throughout. She is described as a graduate student with psychological and linguistic talents, artistic abilities, visual imagery. She is subject to migraine headache, does automatic writing easily and apparently possesses clairvoyant powers. The material used in the four main series consisted of cards, each bearing a printed letter or a point, written either with luminous paste, colored crayons or ink. The cards were covered with cellophane and placed in opaque paper envelopes. In Series II, a piece of black cardboard was placed under the cellophane on some of the cards so that the letters were not visible. The cards were presented to *O* in various ways. In Series I, they were handed to her under a dark cloth, which came up to her neck. She was permitted to take the cards out of the envelopes and handle them. In the later series, the cards were placed in such positions as in a chest on a shelf in front of *O*, on top of a dark cloth, and behind a screen set in back of *O*. In every case, *O*'s problem was to identify the letters without ordinary visual or tactual cues. Introspective reports were taken and these, together with *O*'s drawings of the letters, are recorded. Frl. D. succeeded in naming some of the letters in every series, the number of correct identifications varying from 20 to 50%. Thirty-three of the 134 trials are counted correct, 9 incorrect, and 31 doubtful. The letter-form was correctly described 40 times, incorrectly 21 times. Bender claims that these results cannot be due to chance, for the number of successful trials is approximately 7 times that predictable from chance.

In Series III and IV, the *Hellsehen* technique was combined with an *Aufhellungs* experiment. After the attempts at clairvoyant identification, the same letters were projected at liminal intensity upon a ground glass screen. The form perceptions thus aroused are found to be phenomenally similar to the imagery described by *O* in the clairvoyance experiments.

The "negative series," the results of which are not included in the regular tabulations, was performed between Series II and III. The material for this experiment was prepared by a member of the English Society for Psychical Research, the author having no knowledge of its nature until the end of the series. There were 12 objects, each consisting of a strip of cardboard folded together, with symbols (numerals, geometric figures, letters or words) written on the inner side. Each strip was placed in an envelop wound with adhesive tape. The results were entirely negative. Bender explains that the psychological situation was unfavorable, the delayed verification of the results arousing unpleasantness and nervousness in the *O*, which affected her general mood and bodily state, producing fatigue

phenomena. He thinks the failure may be due either to the nature of the material or a suggestive effect or some temporary indisposition. No protocols or detailed summary of results are given for this series.

In conclusion, the author discusses various theories of supersensory perception. He suggests that the clairvoyance process probably occurs in a state of psychic dissociation, the *Hellsehbilder* originating in a dissociated subconscious mental stratum. The experiences have the character of spontaneous fluctuating *Anschauungsbilder*.

Bender's evidence for clairvoyance is not conclusive. In the first place, he did not rule out the possibility of telepathic transference, as he claims, by having an assistant prepare the cards and hand them to the experimenter. There was nothing to prevent a telepathic transference from the assistant to the *O*, or, according to Bender's own theory, transference from the assistant to the experimenter's subconscious and so unconsciously to the *O*. The nature of the errors lends support to this possibility. In many cases, *O* reproduces several letters similar in sound to the stimulus letter, but entirely different in form. Thus, for example, stimulus V gives rise to successive images recorded as G, B and Z; while, in response to stimulus C, *O* writes P, B, E, and T. The negative results of the one series in which the material was unknown to any of the persons taking part in the experiment also indicates that knowledge may have been unconsciously conveyed to the *O*.

The author's method of tabulating his results gives them the appearance of greater accuracy than they actually possess. He counts any trial correct if the stimulus letter is reproduced (or correctly described) at least once, no matter how many other letters are given before and after the correct one. As many as 18 erroneous reproductions appear in a trial that is counted correct! Such procedure greatly increases the possibility of chance success. Bender's probability calculations are entirely misleading, since they are based simply upon the number of stimuli, with no consideration of multiple responses. Were this factor taken into account, the number of successes predictable from chance approach (and in two series actually exceed) the number obtained by so-called "supersensory processes."

Nyack, N.Y. E. FRANCES WELLS

Psychology and Modern Problems. By MORRIS GINSBERG, C. G. SELIGMAN, and others. Edited with an Introduction by J. A. Hadfield. New York, Longmans Green & Co., 1936. Pp. vi, 241.

The book consists of a series of lectures delivered at the Institute of Medical Psychology, London, apparently more or less supplementary to regular courses in Psychopathology and Psychotherapy. The first two lectures, by Morris Ginsberg on "National Character" and by C. G. Seligman on "Psychology and Racial Differences," are thoughtful but tentative discussions of large areas of human relationships in which scientific psychological investigation is beginning to make its way. The next lecture, by Ramsay Muir on "Liberty, Authority, Democracy," is a brilliant exposition of the principles and ideals underlying the British and American systems of government, and the difficulties, particularly psychological ones, which must be overcome to keep such governments functioning successfully. Crichton-Miller, in his lecture on "Educational Ideals and the Destinies of Peoples," says, "If it is desired to rule a country in a certain way the most important consideration is to maintain the suggestibility of children. . . . In this way, individual discrimination and criticism are kept in abeyance. I submit that it is this suggestibility

which is the real enemy of democracy. In a country where it has been fostered, perhaps in the interests of a dictator, the demogogue finds that exact mentality among the voters which he desires to exploit, and it is exploitation of the suggestible mass of voters which renders democracy feeble and foolish." He also believes that, "education for character is imperative, and not education for a particular political programme or particular national destiny," but he is somewhat puzzled as to the means by which education for character can be effective without prejudicing the child's mind in favor of one governmental or social system rather than another. In fact, it is rather difficult to see how a child can be trained in character without prejudicing him in favor of right rather than wrong. But how to tell the one from the other a good many of this generation, apparently, have found it rather difficult to discover. J. C. Flügel's lecture on "Psychological Aspects of Marriage and the Family," like priestess Diotima in Plato's Symposium, unveils "some of the lesser mysteries of love." Emanuel Miller in his lecture on the "Artist in Modern Civilization," after reviewing modernistic tendencies in several of the arts, says that "the technique of the cinema is perhaps the best means of understanding the artistic tendency of the age. We cannot escape wholly from the mechanical in our present lives, and it is the artist's duty to sieze hold on this mechanism as a means of expression." He sums up a very brief and clear outline of the relations of modern art forms and modern life by saying that "it would be idle to continue to give examples of the outstanding artistic manifestations of the age in which we live, but I think it will be seen that they are definitely interrelated, and illustrate in what way the technical advances of the last fifty years have influenced the emotional moods which characterize our life, and how these in their turn have bent these technical advances to suit those fundamental aesthetic needs which have been the heritage of men since the cave-dwellers first painted their pictures on the caves of neolithic times."

Dean Inge, in the closing lecture on "Psychology and the Future of Religion," reiterates a point that William James made in a similar connection, viz. that, no matter how completely psychology may explain how certain beliefs arise in the mind, the truth of those beliefs does not depend upon the temporal antecedents and efficient causes which usher them into the world, but upon the nature of the reality which the beliefs envisage. To say, as Freud does, that, "God is a father complex," does not, even if true, disprove the existence of a beneficent deity. God's existence and His continuing to preside over the destinies of the universe are not dependent on the manner in which His image may arise in the mind of a child. Moreover, those guilty of this fallacy of presumption are almost always guilty of another, which is, that they all know the nature of reality already, and this nature which they know is very far from being in accord with that signified by the more highly developed religions, and the lives of enlightened and spiritually minded men. Dean Inge concludes with a plea for the coöperation of psychology and religion in the field of worship and in the cure of souls. For, he maintains, religion, "is a normal and healthy activity of the mind, that it is, in fact, a permanent 'form' of the Spirit," and that the aid of psychologists might be very useful, and, "we might expect great help from them in improving the expressions of religious consciousness."

The lectures vary somewhat in merit, and probably also in the amount of their appeal to the different interests of different readers. The trained psychologist will

find them valuable in that they deal, thoughtfully and suggestively, with a variety of topics bordering his field on its different sides, while the layman will find them interesting and informative. They compose a very interesting, suggestive, and readable book.

Hobart College FOSTER P. BOSWELL

The Animal Mind. By MARGARET FLOY WASHBURN. Fourth edition. New York, Macmillan Co., 1936. Pp. xii, 536.

According to the publisher's announcement, the fourth edition of *The Animal Mind* contains 50% new material. This is a gross exaggeration. At the very most only 20% of the material is new. The introductory chapters and those on sensory discrimination, spatial orientation, and perception are practically identical with those in the third edition. The chapter on learning is the only one which has undergone any extensive revision. There are two new chapters, one on higher mental processes and the other on drives, incentives and emotions, but much of the material in these chapters is identical with that found in other chapters of the third edition. The chapter on attention has not been changed, except that the summary which appeared as a separate chapter in the third edition is now a section of this chapter. The bibliography has 548 more items than that in the third edition, bringing the total number of references to 1,683. The extension of the bibliography is the most valuable feature of the new edition.

Miss Washburn writes in the preface that "the principal change in the attitude of investigators of animal behavior since the third edition (1926) of this work appeared is the decay of behaviorism as an interpretation and the revival of animal psychology. The conclusions drawn from experiments are now expressed in subjective terms." This belief results from autistic thinking and a misinterpretation of the new trend in animal psychology. R. H. Waters writes in *Comparative Psychology* edited by F. A. Moss, "The movement represented by this book (Washburn's *Animal Mind*) is now dying out" and according to Warden, Jenkins and Warner's *Introduction to Comparative Psychology,* "The nature of the animal mind is merely regarded as a false problem and one that does not arise in the objective analysis of behavior." Furthermore, two other recent books in animal psychology, Munn's *An Introduction to Animal Psychology* and Maier and Schneirla's *Principles of Animal Psychology* are written from an objective point-of-view. Either Miss Washburn is wrong or these recent books do not truly represent contemporary animal psychology.

Moreover, Miss Washburn's revision is itself proof that the objective viewpoint is more strongly entrenched in animal psychology than ever. She finds it necessary to revise and expand just those sections, learning and motivation, which topics are the least revealing regarding the conscious states of animals.

Miss Washburn misinterprets a recent tendency on the part of comparative psychologists to use such terms as 'reasoning,' 'hypothesis,' 'insight,' 'cognition,' and 'purpose' as indicative of a renaissance of subjectivism. The reviewer sees in the use of these terms an attempt to define objectively, originally mentalistic concepts in order to make them accessible for scientific investigation. They have come to mean now forms of behavior rather than conscious states or processes.

If one disregards the mentalistic approach of Miss Washburn, *The Animal Mind* is a very useful book for both student and investigator. It contains a great amount

of information regarding the comparative behavior of all animals from amoeba to ape. The methodological cautions for animal work should be required reading. The chapters on discriminative behavior are probably as exhaustive as can be found in any other textbook of animal behavior.

University of Oregon CALVIN S. HALL

General Psychology. By WAYLAND F. VAUGHAN. New York, Doubleday, Doran & Company, 1936. Pp. xxi, 634.

The author aims to acquaint the novice with the subject-matter of psychology, and thus to make him capable "of discussing psychological matters intelligently and of planning and executing experimentation in sound fashion"; and also of appreciating "what is meant by the scientific approach to the study of human nature." Much of the book is written in a sprightly manner, intended to be humorous, and sometimes being so.

At first, he finds it hard to decide what the subject-matter is to be. It is to be 'mind,' but what is that? Given a subject S of consciousness; a collection O of objects of consciousness; and a set A of relations connecting O to S, and also interconnecting the members of O through S, these relations being denoted by such verbs as *perceiving, imagining, thinking of, willing,* and generally by *awareness of.* At the outset (p. 10) the author identifies 'mind' with A, thereby making both S and O non-mental, as they are by the postulates of realism, which he sometimes appears to accept. He does not, however, adhere to this definition; scattered through the first 80 pages are many equivocations: *e.g.* between O and A; between A and $(A + O)$; between S and A; etc. These make for grave confusion.

Eventually, however, the author proceeds to discuss human behavior in the conventional manner, explaining it in terms of causal laws of energetics, memory, and purpose, interchangeably, though usually leaving these laws unstated.

One long chapter states many interesting problems of psychology, roughly describes the procedures employed to attack them; and presents many good pictures of special apparatus. This is to show that psychology is not "merely a pseudoscience because it deals with the mysterious forces of mind."

Three chapters on sensory and neuromuscular equipment give the conventional presentation. Most of the illustrations are diagrams, instead of unexplained cuts borrowed from the anatomists. The treatment presupposes more information about vertebrate anatomy, cell structure, and body-chemistry than most sophomores actually have. As is usual the "nervous system" which is used for explaining behavior is an hypothetical nervous system, having such properties as one assigns to it in order to explain the behavioral facts, whether the physical nervous system has those properties or not. As is usual, this hypothetical nervous system is seldom referred to in the later chapters—probably because it has no great explanatory value.

The chapters on determining tendencies (*i.e.* bias, fundamental urges, formation of habits) and on effective adaptation (*i.e.* attending and perceiving, remembering and anticipating, mental conflict, thinking, intelligent adjustment) contain some very interesting material, with many attractive pictures.

Indeed, I find the whole book interesting; and yet I feel that it ought to be thoroughly purged. First, it contains many gross inaccuracies, some of which are likely to disorient the reader. Those which I have detected are far too numerous

to be exhibited here. Secondly, in many places where criticism of the presented material is necessary to orient the reader, the criticism is either feeble or absent. For example, the weaknesses that inhere in the questionary-method are very lightly passed over. There is no mention of the fact that the conventional methods of scoring tests of attitudes, aptitudes, skills, and personality traits presuppose that these data combine according to the laws of numerical addition; and of the additional fact that the presupposition is certainly false. It is not pointed out that at least some phenomena can be "explained" in terms of efficient causation, mnemic causation, and telic causation interchangeably, so that none of these sets of causal laws is necessarily excluded by any of the others; or that all three principles of causal explanation are actually employed in the book.

Finally, the style is not always pleasing. The author can write clearly and lucidly, and often does so. But why call the outer canal of the ear the "external auditory meatus," which "is *the part which* your mother used to wash for you on Sundays"? (p. 129, italics reviewer's). Also, the small boy scratches himself, "because nobody knows better than I do where I itch" (p. 87). This exemplifies the privacy of sensory content. There are many references to ephemeral issues and to heroes of the moment; there is a phrenological chart of the character *Andrew Gump*. I casually counted 23 references to recent newspaper articles, and one to *Psychology Magazine*. Examples of current slang: "So's your old man." Also (p. 145), "in these days of B.O. (bodily odor) and halitosis, . . ." Taste aside, these features, by merely *dating* the book, are likely to shorten its life. I still doubt if one can present the facts and principles of a science so wittily that the sophomore will assimilate them unintentionally while he chuckles.

The format is excellent; the index, fair.

American University H. M. JOHNSON

Der musikalische Schaffensprozess: Psychologie der Schöpferischen Erlebnis- und Antriebsformen. By JULIUS BAHLE. Leipzig, S. Hirzel, 1936. Pp. ix, 253.

Bahle offers an interesting contribution to the problems of cultural creative effort, but his book nevertheless provokes criticism of his principal conception. It is written with the intention of conquering the so-called absolute or autonomic theories of musical creative effort. These are regarded, according to the meaning of the author, as too onesided, the musical productivity overlooking its real growing-out of the natural and social existence of man. Making the attempt of a sort of synthesis of the musical creative production from their environmental and psychic bases, he is—as it seems to me—falling into the contradictional mistake of not acknowledging the real internal laws of musical life as the central aim of any research in the world concerning musical facts. Regarding the results of this book from a more objective point of view, although it deals with musical materials, it gives no real nor concrete enlightening as to the creation of musical products, but only about artistic creative effort in general. Investigation about any special field of art must pay attention to the principal differences between the plurality of the modes of expression of artistic life. They must have as their aims—and that from the beginning of the research—the specifications of the field and its particular laws, in order that they will not only give general esthetic but real concrete results. In the whole book there is nothing to be found about internal musical reality and it is also characteristic that Bahle mentions nothing about the research done in this

line, especially in Anglo-American psychology, referring only to Seashore's important work in the Iowa Institute for Musical Creative Effort.

Dividing his book into three parts, choice of texts (Textwahl), productive process (produktive Erlebnis), and the different kinds of motivation in musical creation (schöpferischen Erlebnis- und Antriebsformen), Bahle is trying to approach musical creativeness in the way of experimental research. Offering six German poems to musicians for composition, together with a sort of test concerning the introspection of their creative activity, he received eighteen songs about his chosen texts and nine contributions of other composers. Regarding this as a new method of testing creative effort, which he calls "Fernexperiment," it may be said that this method is not a new one; it has already been used ten or twelve years ago by W. McDougall and Eliot Hutchinson. According to the inadequate viewpoint described above, under which the whole research is done and which also underlies the test, the outcome does not give real concrete results but only general aspects. For example, in the choice of the text he points out as the primary factor the absolutely unmusical one of pure humanitarian feeling free from all conventionalism, and the psychic background is the world of feeling more than the appeal to the intellect. Texts must have 'space' for the expression in music, aside from their poetical content. The part about the productive process is almost entirely devoted to a discussion of the autonomic theories of musical productivity as they are represented, especially by Utitz, Pfitzner, Gatz, and Kurth. The positive result is only that there exists not a musical productivity disengaged from the human personality, but is an expression of the individual soul, especially of its feelings. The most concrete and therefore the most valuable results are to be found in the last part on the motivation of musical creativeness. They are divided into dynamic and effective (Werkbestimmende). The dynamic are: the geophysical (weather, daytime, etc.), the physical environment (work place, rest, disturbance, etc.), solitude, productive state of consciousness (day-dreaming and relaxation), social reactive conditions (orders, competition, criticism, Maecenas, family, friends), social active conditions (ambition, love, acknowledgment, glory, egoism), the creative will, stimulation. The effective motives are characterized as the productive mood, the productive necessity, the productive vocation and mission, enlargement of efficiency and concentration and of selfconsciousness. Finally he discusses the problem of abnormality of the artist. This entire account is based on reviews of letters and memoires of the most famous musicians. This is the greatest value of the book. In adverse criticism it may be said, however, that Bahle ignores absolutely the differentiation of musical types, which since the time of H. Nohl has been one of the most important keys to the psychology of musical productivity.

Baltimore, Md. ERNST HARMS

Will Therapy: An Analysis of the Therapeutic Process in Terms of Relationship. By OTTO RANK. Translated by Jessie Taft. New York, Alfred A. Knopf, 1936. Pp. xxv, 291.

Truth and Reality: A Life History of the Human Will. By OTTO RANK. Translated by Jessie Taft. New York, Alfred A. Knopf, 1936. Pp. xii, 191.

These two translations, published in July of this year, may popularize in America the views of another of Freud's students, who when his originality asserted itself, was forced to depart from the company of his old master. The first book consists of translations of Volumes II (Die Analytische Reaktion) and III (Die

Analyse des Analytikers) of Rank's original *Technik der Psychoanalyse* and a digest of Volume I (*Die analytische Situation Illustriert an der Traumdeutungstechnik*) by the translator. Volume I has been presented in digest because it was written in the context of orthodox Freudian psychoanalysis, appearing in 1926 before Rank realized the fundamental divergence of his views from Freud's. The second book is a translation of the third volume of Rank's *Grundzüge einer genetischen Psychologie auf Grund der Psychoanalyse der Ichstruktur*, and represents, in the words of the translator, "Rank's unique contribution to psychology and philosophy." The argument in both books is presented against a background of Freud's metapsychology, and serves, therefore, as a critique of his psychoanalysis.

It is impossible here to trace the divergence of Rank's views from those of his master. Suffice it to say that in his attempt to resolve what he considered to be the incompatibility of theoretical and therapeutic interests in the orthodox psychoanalysis, he arrived at new view and use of 'resistance,' a manifestation in the therapeutic situation of what Rank calls 'will.' This resistance has been neglected by Freud except as it has been considered an obstacle to obtaining early infantile fantasies. Adler recognized it clearly as a battle for supremacy and called it the masculine protest, and for him it became the fundamental motive. For Rank this resistance becomes the key to therapeutic value in the analytic situation. Ideally the basic rule of 'free association' would eliminate any self-direction by the patient, and the analyst must guard against forcing either prohibitions or commands upon his patients. The fact that this is possible only to a limited degree, according to Rank, saves some therapeutic value from the orthodox psychoanalysis, for every attempt to exclude this resistance (will reaction) actually tends to strengthen the patient's will. Taking this opposition between patient and therapist as the fundamental feature of the analytic situation, Rank attempts to control it for the patient's good. While there are many ramifications which alter psychoanalytic theory considerably, Rank's contribution results primarily in what is called an active technique of analysis. The period of treatment is shortened: a limit is set on the time of the analysis in advance. These results strike at some of the fundamental weaknesses of Freudian psychoanalysis as a socially useful therapeutic device. The proof of the pudding, of course, is in the eating, so a valuation of Rank's method must rest upon some sort of comparison of his therapeutic results with those of Freud and the other analysts. At least the cost to the patient should be reduced.

In interpreting his concept of the will (*Truth and Reality*) Rank becomes frankly a philosopher. He delves broadly into ethics and epistemology as well as the psychology of living. His quarrel with Freud rests primarily in an opposition to the stress laid by Freud upon the rôle of external factors on the development of the individual. In Rank's words, "this is based essentially on the conception that the inner world, taken in from the outside by means of identification has become in the course of time an independent power, which in its turn by way of projection, so influences and seeks to alter the external, that its correspondence to the inner is even more close. This relation to outer reality I designate as creation in contrast to adaptation, and comprehend as will phenomena" (p. 7f). Thus it would appear that free will or something more or less equivalent to this old philosophical concept is introduced into psychoanalysis. Rank is much interested in the artist, the creative man, and neuroses are regarded as miscarriages of this creative will.

Rank's thought must be provocative for anyone interested in psychoanalysis,

indeed for anyone interested in the meaning of human living, but as science the experimental psychologist will find it suffering from many of the same deficiencies found in Freud's metapsychology. I refer principally to a different conception of proof from that entertained by the experimental psychologist. Aside, these differences in conception of proof cry for a logical analysis, something this reviewer has not yet seen.

Both volumes are attractively done. On the whole the translation is good, but one sometimes finds sentences in which the flavor of German stands out too prominently.

Brown University J. McV. HUNT

Adult Intelligence: A Psychological Study of Test Performances. By THEODORE WEISENBURG, ANNE ROE, and KATHERINE E. MCBRIDE. New York, Commonwealth Fund, 1936. Pp. xiii, 155.

This monograph presents the results of a battery of mental and achievement tests administered to 70 mentally "normal" hospital patients who served as controls in a study of aphasic, apraxic, and agnosic patients of the same hospitals. The tests, "chosen to meet the problems of the aphasic study," consisted chiefly of those in common use with children. They required 10 to 15 hr. over a period of about two weeks. Except for some minor sequences, no definite order was followed in giving the tests.

The data, presented in 31 tables and 14 graphs, are difficult to report briefly or to interpret clearly, especially with reference to age-levels of performance, because of the small numbers of *Ss* and irregularities of distribution in the various age levels with respect to sex, education, and mean test performance of the groups from which the *Ss* were drawn. These irregularities all appear to militate against the oldest group, 50-59 yr. old. Yet in performance on 11 language and arithmetic tests tabulated (Table XXIII, p. 73) by age-levels, the 50- to 59-yr.-olds surpassed 20- to 29-yr.-olds in four tests, equaled them in one and fell only one point below the younger group in another.

One test (vocabulary) shows a reliable positive correlation with age whereas seven tests (verbal analogies and non-language) show reliable negative correlations with age. Though manifesting a disposition to minimize these differences, the authors conclude that from the contributions of the various studies in this field "it seems clear that the average level of so-called general intelligence and the characteristics of its course with age have been fairly well established" (p. 123).

Yet all of these studies have been carried on by means of tests which were designed for and standardized upon children and young adults. Such tests may or may not be fair measures of late adult intelligence. Ordinary caution forbids the assumption that they are fair and adequate until controlled experimental studies prove them to be so. No such proof has yet been published to the reviewer's knowledge. The notable failure of these tests so to function with children and young adults, for whom they were designed, as to enable psychologists to agree upon the average age at which intelligence reaches maturity, should cast serious doubts upon their efficacy as indicators of the retention or decline of such mature capacity.

One of several valuable contributions of this study is the indication that, with the exception of analogies, the tests which show reliable declines with age are those (non-language) in which professional and business groups and skilled

laborers do not excel, whereas the tests which are well maintained or which even show gains with age through the fifties are those in which professional groups show marked excellence.

In reacting to the analogies tests here involved, the reviewer has noticed the occurrence of much relevant concrete imagery. If such imagery is really helpful, as it appears to be, its reputed decline with growth of facility in abstract thinking might well be responsible for the early and continued decline in performance on the particular analogies tests employed by the present and previous investigators. In this case no decline in intelligence would be indicated by the results of these tests.

Kansas State College J. C. PETERSON

The Relation between Morality and Intellect. A Compendium of Evidence Contributed by Psychology, Criminology, and Sociology. By CLARA FRANCES CHASSELL. New York, Bureau of Publications, Teachers College, Columbia University, 1935. Pp. xi, 556.

A statistical evaluation of many varied forms of evidence bearing upon the relation between morality and intellect is presented in a very systematic manner. Part I consists of a review and synthesis of correlational and non-correlational evidence, the latter summarized in tabular form and related by coefficients of colligation. The results of about 300 investigations are brought together in this section. Paired delinquent and non-delinquent groups are compared in their performance on various intelligence tests, their school achievement, and percentage illiteracy. Studies of delinquency among the feebleminded and mental deficiency among criminals are carefully evaluated.

In Parts II and III various studies of measures and ratings of morality and conduct as related to measures and ratings of intelligence are summarized and evaluated statistically.

Finally one arrives at the conclusion that the relation between morality and intellect in restricted groups is variable but clearly positive, the correlations usually falling between 0.10 and 0.39. In the general population this relation is higher but probably below a coefficient of 0.70.

This book is an interesting study in a methodology of synthesis of a great amount of evidence from widely varied sources but provides practically nothing of interest or value to the clinical psychologist or educator who is faced with individual problems of human social adjustment. The conclusions reached may have an air of mathematical precision but the entire book contributed very little, if anything, to our genuine understanding of the relation between morality and intellect.

A selected bibliography of 337 references is given.

Emma Pendleton Bradley Home HERBERT H. JASPER
East Providence, Rhode Island

General Psychology. By DAVID BALLIN KLEIN. New York, Henry Holt & Co., 1936, Pp. xiv, 560.

As its title indicates, the book differs both in intent and in content from the earlier text by Perrin and Klein. It contains more of problem and theory and less of established fact than do most other recent introductions to psychology. For example, it treats at length structuralism, configurationism, behaviorism, psycho-

analysis, and minor systems. (Professor Klein calls his own point of view "systematic eclecticism.") Spranger is given several pages and the Würzburg contributions are given fourteen. On the other hand, the book contains no details concerning reaction-times, rates of forgetting, maturation experiments or sensation, to mention only a few of the kinds of experimental data eliminated to relieve presumably, the 'cultural' student. Psychophysiology is almost completely ignored. An inventory of sensorimotor mechanisms takes up 25 pages but the glands and the central and autonomic nervous systems are hardly mentioned, while many pages are filled with such problems as the mediacy of knowledge, the consequent importance of the introspective method, and the definitions of terms (although 'psychology' is itself never defined, on the grounds that a beginner would not understand a technical definition and a final definition would be superfluous at the end).

To be sure, there is considerable factual material in certain fields. The sections on motivation, learning, and cognition are very thoroughly done and would be of value even to advanced students. The style is excellent. The volume is easy to read and holds one's interest throughout.

The book is an admirable 'general psychology' for beginners. The only question is whether beginners, and especially those who proceed no further in the subject, should be started off with theory before they get the facts. While the treatment of certain subjects (particularly psychoanalysis and social motivation) are especially well suited to the types of students for whom Professor Klein is writing, the inclusion of so much controversial material in an introductory text tends to defeat the author's ends and to send the transient students away with a picture of a nebulous mass of fact and theory.

Mount Holyoke College SAMUEL P. HAYES, JR.

Education and the Psychology of Thinking. By PERCIVAL M. SYMONDS. New York and London, McGraw-Hill Co., 1936. Pp. xii, 306.

The emphasis in this book is placed upon the thinking *processes* as a field for research and educational efforts. The discussions are, in part, based on introspective reports.

To be sure, the author has apparently made too great demands on the introspective ability of his observers. In each case the introspective reports seem to have been delayed until after the completion of the thinking process in question. More adequate introspective methods are available which will throw light on important individual differences in mental operations. Such methods should be used as a stimulus and supplement to "objective" research. Owing to the limitations of his method the author's discussion is confined to a general survey of the forms of thought and he admits "that this method is nothing more than suggestive and that the observations are little better than hypotheses." In fact, much of the text is elementary logic. Moreover, "the results are limited to the extent that the only kind of thinking discussed is that which can be done with words in response to a verbally phrased question." Nevertheless, the book will stimulate the interest of American investigators in a more thoroughgoing psychological and educational study of the highly differentiated intellectual operations.

The author's outlook is primarily that of the educational psychologist. "Some psychologists have taken the point of view that thinking is relatively immune to education." The author is not in agreement with this belief: "Thinking is an

achievement to be won—it is a product of education." "It is a truism that children will forget many of the facts that they learn in school but it is our hope that they can learn a method of meeting problem situations which will result in greatly increased resourcefulness, creativeness, and improved sanity in human relations." "This book shows something of the task that education faces in increasing thinking ability." "It is safe to say that teachers will not make genuine progress in teaching pupils to think—independently and surely—until they themselves know something about the thought process." "They must be as interested in the process of thinking as in the outcome of thinking."

Wayne County Training School THORLEIF G. HEGGE
Northville, Michigan

Charakterologie. By PAUL HELWIG. Leipzig and Berlin, B. G. Teubner, 1936. Pp. vii, 295.

The present book aims to present a survey and critique of the field of modern general characterology. It expressly limits itself to modern characterology as contrasted with the pre-modern doctrines, to general characterology as contrasted with the special characterologies (which deal with special groups such as nations, races, and vocational groups) and to German characterology as contrasted with non-German doctrines. Classification is regarded as one of the main problems in the field, and the notion of type is considered to be one of the basic concepts. This concept is held to play an outstanding rôle not only in the typologies but to permeate all fields dealing with character.

The book begins with a general section in which are discussed the concept of character, its relation to "the Psyche" and the peculiarities of the methods of investigation in characterology. Following this introduction, each of a series of doctrines are described and critically examined. The doctrines of characterology are classified under three main headings: typologies, structural systems, and explanatory systems. The typologies attempt to classify the varieties of character according to type; the structural systems describe character as built up on the basis of fundamental components, such as drives, directions of interest, modes of striving, and so on; the explanatory systems seek to derive character from non-character factors.

Among the typologies are considered the non-systematic doctrines, such as those of everyday life, the systematic typologies (Schiller, Nietzsche, Spranger, Jung) and the medical typologies (Kretschmer, Jaensch). Among the structural systems, that of Klages is considered in most detail. Among the explanatory systems, the author deals with the psychoanalytic doctrines of character (Freud and Jung), the characterology of Individual Psychology (Adler), and with the doctrines of inheritance of character. The descriptions of the various doctrines considered are generally clear and well outlined. Many of the sections are accompanied by extensive bibliographies.

The Training School, GEORGE KREEZER
Vineland, N.J.

Scenery and the Sense of Sight. By VAUGHAN CORNISH. London, Cambridge University Press, 1935. Pp. xii, 111.

It is rare one finds a book such as this, containing accurate and penetrating descriptions of natural events by one who is not a professional psychologist. For many

years the author of this book has been concerned with natural phenomena which require an observer for their description and explanation, *e.g.* changes in apparent size of sun, moon, and constellations, size and shape constancies and departures therefrom, apparent curvature and dip of the horizon, number of rays in stars, appearances of sky, clouds, and landscapes under changing conditions, emotional effects of colors and changing patterns, conditions of lustre and translucence. The book abounds in observations and descriptions, not to be found in any psychological treatises, which should be investigated by the psychologist with all the means at his disposal. On the other hand, due to unfamiliarity with the contributions of psychologists, a number of observations are wrongly accounted for. Thus in criticizing the view found in treatises on perspective that the plane of a scene is always vertical in vision, the author generalizes, incorrectly, that "we instinctively regard an object as extended in the plane at right angles to the line joining the object and the eye" (p. 61). This generalization holds only for scenes which have a predominantly filmy appearance, since we know that surface colors may inhere in any plane, as they tend to approximate the plane of the physical object.

The book contains 10 illustrations by the author which give the reader an idea of the method used to measure apparent size, form, and distances. Drawings were made at various times of a given scene and then compared and measured. Repeated drawings of the same scene under identical conditions, even after long intervals, proved the author's subjective scale remained fairly constant. Hence he was able to make safe inferences about changes in magnitudes from his drawings.

For a word delineation of natural beauties hardly to be found outside the greatest poets and prose writers, for a most unusual combination of esthetic appreciation and faithful description, and, of most importance to the psychologist as scientist, for phenomenological observations which are new and merit experimental investigation, the reviewer recommends this most unusual book.

Bryn Mawr College HARRY HELSON

Psychology of Sex. By HAVELOCK ELLIS. New York, Emerson Books, 1935. Pp. ix, 377.

This single-volume work, which bears the subtitle "A Manual for Students," is intended by its author for those who, for one reason or another, do not wish to set themselves the task of reading the seven volumes of his larger treatment of the subject. Although evidence is sometimes adduced from studies of markedly abnormal individuals, the chief concern of the volume has to do with the normal phenomena of sexual behavior. In this general frame the author carries the reader through an introductory chapter on the biology of sex, through succeeding chapters on the sexual impulse in youth, on sexual deviations and symbolisms, and on homosexuality, to concluding chapters concerned with marriage and the art of love.

This volume is not a mere boiling down of previously published writings by the same author. It stands in its own right as a well-rounded, if abbreviated, account, and as an account which has been brought up to date by many references to the recent literature of the subject. The literary style is fluent and never pedantic; the author exhibits without ostentation an encyclopedic knowledge of the publications of his contemporaries. The careful reader will observe, however, that the volume is marked by characteristic contradictions. The same Havelock Ellis who exhibits a wise and understanding tolerance for forms of behavior commonly tabooed does not hesitate to be dogmatic in choosing between rival theories. The Ellis who skil-

fully dissects faulty logical structures in current popular opinion is often unduly impressed by small statistical differences of unknown reliability. And the Ellis who militantly protests against credulous acceptance of folklore is frequently willing to generalize from small and localized samplings. Havelock Ellis, it may be remarked, writes always as a naturalist rather than as a scientist.

Although psychologists may inveigh against such shortcomings of the volume as these, they will welcome its candor, its wisdom, and its insightful tolerance in a field so generally marred by evasion, superstition and hypocritical myopia. They will know many who could read with profit Ellis' treatment of the deviations from 'normal' sexual behavior; and they will realize that there are few who could without profit read his sections on marriage and the art of love. One suspects that its catholic review of the literature and its sanity of exposition will render the volume important to psychologists long after its methodological shortcomings have been recognized and forgiven.

Cornell University JOHN G. JENKINS

The Psychology of Christian Personality. By ERNEST M. LIGON. New York, Macmillan Co., 1936. Pp. xiv, 392.

"It is the purpose of this book," Professor Ligon says in the Introduction, "to consider the teachings of Jesus in the light of the psychology of personality. To do this will require the setting forth of three sets of facts."

"In the first place, it will be necessary to describe, as accurately as possible, the Christian personality. This means that the various teachings of the Sermon on the Mount, which will be used as the basis of Jesus' teachings, will have to be interpreted. In the second place, these teachings will be examined psychologically. That is, the Christian personality will be evaluated in terms of mental health and strong personality, using the principles of mental hygiene, psychiatry, and psychology. Finally, assuming that Christian personality is mentally healthy and strong, how can it be developed?"

The point of view of the book is that of a competent, healthy minded psychologist, who is also a Christian, applying the teachings of his science to the problems involved in the program which is outlined above. In the reviewer's opinion he has succeeded admirably in the carrying out of his plan, and has produced a book of lasting value, and perhaps of significance in the history of the psychology of religion. It is addressed particularly to parents, to the clergy, and to men and women everywhere who seek in religion the composing and strengthening of their souls. It is also of decided interest to the psychologist for the manner of its applications of psychology to the problems involved.

Since its scope is rather strictly limited to the development of personality, the book is non-theological in the sense that it makes no attempt to explain how Jesus and his teachings fit into the world as we also know it from secular sources of knowledge. In fact Professor Ligon depreciates all deductive thinking by which alone it would seem that, in the present state of our ignorance, such explanations might be attempted. He assumes that "mental health and strong personality" are the highest good of human life, and makes out a strong case that the teachings of Jesus as elucidated by psychology in his book will enable one to achieve this aim. Experimentally and pragmatically any path leading toward this goal is valid; any path diverging elsewhere is erroneous.

Professor Ligon writes, not as a theologian, but as a psychologist; nevertheless,

the universal significance of the principles involved in his discourse force the question upon any attentive reader, "Are these the principles upon which this universe is built, or has it been constructed upon others?" Any attempt to answer such a question would involve deductive reasoning and forms of logical inference that transcend the scope of the experimental pragmatism, which, Professor Ligon holds, is the one scientific method for the discovery of truth.

Hobart College FOSTER P. BOSWELL

The Natural History of Mind. By A. D. RITCHIE. London, Longmans, Green & Co., 1936. pp. viii, 286.

The Natural History of Mind is based on a naturalistic approach to the study of the behavior of human beings in their environment. In the beginning of this volume the author states that his aim is to see how far it is possible to study mind in a purely naturalistic way. He begins his discussion by going directly into the problems of dualism *vs.* monism, of matter and mind, of vitalism *vs.* mechanism and of scientific knowledge *vs.* scientific theories. After an interesting philosophical and comparative treatment of these problems he discusses the problems of *causation* and *life*. The approach to these first three chapters though philosophical in nature, gives special consideration to materials from the physical and biological sciences that may be used in establishing a better understanding of the theoretical items under consideration.

Both the philosophy and physiology of Descartes are considered. Present day knowledge of the structure and function of the nervous system is discussed rather adequately. The importance of the fusion and conditioning of the reflexes, emotions and even thought activities is pointed out. The problem of, What does Psychology Study? is presented in Chapter VI. He says that "when we try to look into the mind we see the external world and its reflections . . . to see into the mind itself to comprehend its acts the best way is to look outward to the behaviour of other people and to the interactions of their behaviour and our own. Human behaviour, then, is the only special subject matter of psychology" (p. 159). The effects of physical things are minimized in so far as they effect man's behavior, because "in the end . . . the study of mind is the study of man's behaviour in his environment, an environment that consists of other persons before it consists of things" (p. 278). "If the behaviour of men is explicable at all the explanation must be mainly in terms of social factors" (p. 278).

Although no references are listed and no citations made, this volume presents a scholarly treatment of the meaning of psychology and presents materials from a large number of the eminent philosophers who have contributed to the development of present day psychology. The book can well be said to be interesting and scholarly and a valuable volume of source material that presents man as a physiological creature that must be studied as a whole but can never be adequtely understood without the use of a philosophical approach.

University of North Carolina K. C. GARRISON

Ganzheit und Struktur: Festschrift zum 60. Geburtstage Felix Kruegers. By OTTO KLEMM and others. Munich and Berlin, C. H. Beck, 1934. Pp. vi, 214; 8 plates.

This volume, written in honor of Professor Krueger's sixtieth birthday, is made up of articles by his pupils and friends. These articles intend to prove that the

concepts of the "Leipzig School" can be made the basic ideas in all fields of psychology.

In a first article Hans Volkelt presents the main principles on which the "Totality Psychology" (Ganzheitspsychologie) is based. These principles are three in number. (1) The principle of the whole. Psychic phenomena and mental life in general are not aggregated. Every psychic event is a whole possessing at each moment only one quality ("Gestalt" quality, "Komplex" quality). Out of such wholes, subunits as dependent parts may eventually emerge. (2) The principle of the central significance of feeling in mental life. (3) The principle of structure. Behind all human behavior there is the psychophysical (unconscious) "structure," which is the organized totality of the dispositions in the individual. Structure therefore determines the overt phenomena of human life.

Those fundamental ideas of the Leipzig Gestalt School are, in a rather general and abstract manner, carried through the different fields of psychology. Arnulf Rüssel discusses the psychology of perception, Otto Klemm the psychology of performance, Rudolf Hippius the psychology of thought, Ehrig Wartegg the psychology of emotions, Johannes Rudert the psychology of will.

One of the very few articles in the volume which bring some new facts is that by Adolph Ehrhardt on types. Ehrhardt accepts the system of Sigaud who, as one knows, distinguishes four bodily types: (1) the digestive, (2) the muscular, (3) the respiratory, and (4) the cerebral. Experiments have been carried out with 500 adults and 1400 adolescents. The color-form experiment yields the following results. Type 1: form and color, although both are perceived in a mediocre way they are perceived so equally. Type 2: preference of form over color. Type 3: preference of color over form. Type 4: superior achievements in the apprehension of color as well as of form. There are other experiments testing space perception, technical capacity, ordering of figures, ability of combination, intellectual versatility. According to personal analysis and achievements in the experiments, the 4 types are characterized by the following qualities. Type 1 is practical, open to new impressions, genial. Type 2 is directed toward a single and specialized activity, operates more with the aid of his intellect, and is socially aloof. Type 3 is the emotional type; is not superior in intellect. Type 4 is the highly differentiated type of great intellectual achievement. The statistical data in this article are very abbreviated.

An article on Music by Albert Wellek and one on Community by Graf Dürckheim concludes the volume.

Felix Krueger is doubtless one of the leaders in modern Gestalt psychology. One had, therefore, only wished that the importance of the movement which he founded would have been indicated more by concrete empirical investigations rather than by essays of a mostly half philosophical and abstract character.

Harvard University HEINZ WERNER

The Psychology of Radio. By HADLEY CANTRIL and GORDON W. ALLPORT. New York and London, Harper & Bros., 1935. Pp. x, 276.

This book is the first attempt made by psychologists to analyze the problems of radio broadcasting and the effects of the radio upon social attitudes and social relationships. It is one of the most significant contributions yet made to the solution of social problems that must be attacked by experimental and psychological

analysis if psychology is to function in the direction of complex social adjustments in living that are necessary in an advancing civilization.

The book is divided into three parts. Part I is a summary and interpretation of the general psychological and cultural factors that determine radio programs and the responses of the radio audience. Part II reports five experimental investigations of various mental processes of radio listeners: (1) the ability of listeners to judge traits of personality from hearing the voice of the speaker; (2) the preferences of listeners for the voices of men and of women; (3) the effectiveness of a speaker when heard over the radio in comparison with his effectiveness when heard in the face-to-face audience situation; (4) the effectiveness of the radio in comparison with newspapers, magazines, and books in influencing public opinion; and (5) the methods of organizing material for broadcasting. Part III gives a practical interpretation of the experimental results for the layman and the professional broadcaster.

An analysis of the attitudes of listeners toward educational programs shows that the educational programs in general make very little appeal because the people directing them are not cognizant of the procedures necessary for the successful use of the radio. This phase of the study should prove to be of especial value to the educators who wish to utilize the radio—perhaps our most effective agency for adult education.

Since the radio serves different avenues of social communication than those of other agencies, it seems inevitable that it will be a permanent means of social participation. This pioneering attempt to analyze the social effects of the radio is of major importance to all students of problems of social welfare.

Georgia State College for Women EURI BELLE BOLTON

Normative Psychology of Religion. By HENRY N. WIEMAN and REGINA W. WIEMAN. New York, Thomas Y. Crowell Co., 1935. Pp. x, 564.

It is apparent at the outset that this is not the traditional psychology of religion which describes, compares and analyzes experience and behavior commonly recognized as religious. It might even be questioned that it is psychology at all, since the term, normative, as used in the title refers to values not averages.

The major theme of the book is that religion is adjustment to the universal "connections between activities," or as reinterpreted by the present writer, the total organization of organic and inorganic processes. Religion is devotion to the supreme values for all mankind. Supreme value is "growth of connections between activities which makes them mutually sustaining." Supreme value is superhuman (not supernatural) since its source is beyond the individual, and is symbolized by the term, God. Religious adjustment is a matter of achieving "connections," or in other words, a personality integrated to its total setting, spatial, temporal, and social. Sin is frustration of this growth. Prayer is a process of coördination or connection of interior with exterior activities. All this, of course, suggests the old philosophic theme of organization.

If this program is accepted it is clear that a radical shift in the traditional religious territory must be allowed. Sacerdotalism is abandoned and the field of mental hygiene invaded. A religious attitude toward the supreme social values is held necessary for mental health. Apparently "mutually supporting activities" within the individual, and between individuals, means about the same as "integrated personality" as used by psychotherapists.

No experimental or observational studies are reported but a method of approach to such studies is suggested. If it is accepted that religion and value are matters of dynamic organization then a basis is provided for research which would consist in discovering "connections" or correspondence between functions. The extreme emphasis on connections suggests to the writer that the authors have perhaps fixated so intently upon the links in the dynamic organization that they have overlooked the total reality. Like the blind man in the fable, they have mistaken the tail for the elephant. One aspect of mother, and God also perhaps, is "growth of connections between activities" but it hardly seems to constitute the whole of either. Perhaps the authors might choose to reverse this charge *in re* organization. Their valiant struggle to escape from the spell of archaic categories helps to make the book important.

The text is characterized by a prolix zeal which arouses ideational enthusiasm but is dangerous in that it tends to blind both the authors and their disciples to the sins of nominalism and circumlocution. The more general reader will probably not succeed in mastering the peculiar terminology, and will therefore fail to appreciate the very real contribution in working out the social implications of the organization theme. After all it is more probable than not that men should have emotional drives toward the more general patterns of life activities. Regardless of whether such motivation is religious it nevertheless constitutes an important problem in social psychology.

University of Colorado THOMAS H. HOWELLS

The Anatomy of Personality. By H. W. HAGGARD and C. C. FRY. New York and London, Harper & Bros., 1936. Pp. xi, 357.

In this book, which is written essentially for the lay reader, the authors present a concept for the structure of personality. They never state clearly, however, just what they mean by the term personality. The reader is frequently reminded that personality is not the same as character, since character is a product resulting from the action of the environment upon the personality. He is taught to think of personality as an inborn trait or traits which remain constant throughout life. Such a doctrine gives a fatalistic cast to the book. It is, however, quite in accord with the biological concepts from which the authors derive many of their ideas. Heredity is given undue importance, and many factors are considered inheritable according to the Mendelian schema when, in fact, such assumptions are quite unjustified in our present state of knowledge. The drive to force as many facts as possible into the biological schema results in the writers treating as matters of personality (and therefore inborn) traits which most psychologists and psychopathologists classify as due to environment. The critical reader cannot help but feel that, in spite of the frequent mention of the influence of environment, little more than lip service is actually paid to it.

The bulk of the book consists of an exposition of the five factors that make up personality: physique, impulse, intelligence, temperament, and ego. These sums are not used entirely in their usual senses. Ego, for example, has here a somewhat different connotation from that found in the psychoanalytic literature. The discussion of physique is based entirely on the concepts of Kretchmer. His correlations are accepted quite naïvely, to such an extent that, for example, pyknic habitus and manic-depressive behavior are treated as thought they were interchangeable

terms. Body-build is, therefore, an integral and important part of the personality. One is reminded of the popular books of a decade or so ago which based personality upon endocrine activity. Styles change in theories, but the methods of the popularizers are easily adapted to the change.

The exposition of the five factors is carried out largely by means of delineating types. The writers are medical men, among whom typology is popular because of their training in dealing with disease entities. They apparently fail to realize that human qualities vary continuously over a broad range, even though they rather reluctantly admit the existence of persons intermediate between the types. Perhaps a popular treatise requires a description of types in the interests of clarity, but the reviewer feels that it gives a false impression and leads the reader to incorrect ideas. Each type is illustrated by one or more case histories.

The work is written in a freely flowing style. It is light and easy to read. Aside from some pessimism that may result from the stress on the unchangeability of personality, it is unlikely to have any harmful effect on the lay reader, so far as his own problems and conflicts are concerned. It may give him a false idea of modern psychology and psychiatry. It is an interesting book, but it can hardly be called a scientific work.

Milwaukee County Mental Hygiene Clinic GILBERT J. RICH

Begabung im Lichte der Eugenik. By JOSEF SOMOGYI. Leipzig and Vienna, Franz Deuticke, 1936. Pp. 518.

Among European scientists, more, probably, than in America, investigations in the field of "philosophical anthropology" have received of late an ever increasing prominence. The present volume is one such study. Somogyi's book (a revised and enlarged edition of an earlier Hungarian publication) undertakes to examine the biological, anthropological, sociological, and psychological conditions of special capacities (endowments: *Begabungen*) from the simplest and least noted, up to and including genius in all its manifestations. Systematically this program requires first an extended inquiry into the problems of inheritance (Mendel's Law, Darwin's theory, etc.). Studies of mental and physical inheritance are reported and evaluated. The topics discussed in this Part include: Endowment and The Nervous System, Endowment and The Influence of Hormones, Endowment and Bodily Constitution, Endowment and Race.

It is not until Part II, pp. 219-345, that Somogyi reaches "The Psychology of Endowment." Here, ruling out such talents as athletic and other physical capacities, he limits his subject to cases of *intellectual* endowment: a clever man, an exceptional child, etc. He admits, however (p. 220), that scientific terminology has not yet distinguished between endowment, aptitude, and ability. Be that as it may, Somogyi is concerned primarily with the inheritance, and not with the acquisition of capacities. In practice one must investigate total ability and subtract training: the remainder will be endowment. Like Spearman and W. Stern the author believes that the acquisition of special capacities is conditioned by the person's underlying general ability or endowment. Part II closes with an exposition of various aptitude testing methods.

The Sociology of Endowment is the title of Part III. Some chapter headings are: Endowment and Social Selection, The Question of Equality Among Peoples and Races, The Fate of Especially Endowed Individuals and Families. A final word is

spoken from the ethical point of view: far from standing outside ("above" and free from) ethical obligations, the man of special endowment, more even than anyone else, is *obliged* to live an exemplary life.

For psychologists this book contains little. Its cargo of information is light, its load of interpretation very heavy. Possibly the assembly in one volume of such divergent themes will prove useful to some students. On the whole, however, it does not treat any one topic sharply and concisely enough for the present reviewer's taste.

University of Arizona WILLIS D. ELLIS

So You're Going to a Psychiatrist. By ELIZABETH I. ADAMSON. New York, Thomas Y. Crowell, 1936. Pp. xi, 263.

The expressed purpose of this volume is to "present in simple language an accurate account of modern psychiatric thought." If the reader is an uncritical and devoted follower of Freud he will be thoroughly satisfied with this exposition of Freudian psychoanalysis, complete with quotations from *Hamlet* and *Macbeth.*

The title is irrelevant as the book is directed not so much to persons who are seeking consultation with a psychiatrist as to parents. The chief emphasis is upon the rôle of childhood experiences in determining later behavior, and parents are informed of the far-reaching evil effects which may result from their unintelligent government of the child. The warning to selfish and dominating parents is quite in order, but the Freudian terminology and some of the author's fanciful language obscure the discourse. There is no good reason why one who is trying to avoid technical terms should relapse into such statements as this: "Traitorous messages descend the secret staircase between head and viscera."

The author's concept of 'mind' seems very unclear. At one time it is defined in purely behavioristic terms and again she says "the troubles that overburdened the mind have been in part shifted to the body."

One of the major Freudian mysteries is the relation of mind, with its three levels, to the body and especially to the nervous system. If the unconscious is so closely related to the nervous system that thought and action are determined through it, what happens in the nervous system when, as the theory postulates, the unconscious becomes conscious and ceases to be a powerful governor? How can the psychoanalyst account for the obliteration of previous effects of the environment by merely making the individual see what those effects were?

The author's experience seems to have suggested to her that psychoanalysis is, in practice, a difficult and not always satisfactory therapy, and that psychogenic disorder is much more easily prevented than cured. This is the only real contribution of the book and it is not new.

Cornell University MARTHA LOU LEMMON

My Experiments with Death. By R. DE BARY. London, New York and Toronto, Longmans, Green & Co., 1936. Pp. xvi, 192.

The reader of this book is likely to be somewhat at a loss as to how to take it. If one follows it literally, the material is at once seen to be unscientific and mystical—but perhaps the author meant the work as an allegory, from which point of view it makes better sense. His theme is that each human being has a part in a larger, immortal life. Death becomes an entry of the individual soul into the col-

lective soul of mankind, and is thus the means by which the social purpose of the race is realized. This thesis is supported by an affirmation of the reality of telepathy, by the postulation of a "social nerve" as the special way in which human beings have undergone mutation from the lower forms of life and by the use of dream experiences. The writer's dreams, which he calls his "night life" are described in great detail as "experimental studies." They are given definite objective validity, and the psychoanalytic idea, that they refer only to the motivation of the individual, is rejected.

The book thus becomes an interesting study of one person's dreams, which might provide material for some enterprising analyst. Or, it might be considered as a mystical elaboration of the ancient idea of immortality by works, with the addition of a tinge of the Jungian doctrine of the racial unconscious. Beyond this, however, it does not go. The mystic cannot be a scientist, and his attempt to be scientific approaches the bizarre.

Milwaukee County Mental Hygiene Clinic GILBERT J. RICH

Bible vs. Modernism. By A. N. TRICE and C. H. ROBERSON. Nashville, Rock City Pub. Co., 1935. Pp. xxvii, 289.

The widespread influence and acceptance of the doctrines of "Freudism" is the direct consequence of the general abandonment of belief in the verbal inspiration of the Bible. The modern crime wave is also attributed to the same cause. "Freudism," as the authors term it, is described in a few paragraphs which present a highly misleading picture of psychoanalytic theory concerning the human personality. There is no reason for supposing that either of the authors ever read any of Freud's own writings. Apparently all of this section is re-presented from one secondary source. Evolution is thrown into the discard as crude speculation now wholly disproved; and the work of such devoted scholars as Kent, Eliot, Breasted, Mathews, Moffat, Hocking, Irwin, and Goodspeed is said to be permeated with a desire to overthrow the Bible and faith in God. The proof text method, according to these particular authors' interpretations of the texts, is placed above the inductions of any amount of historical research. Such a book as this would merit no attention from psychologists were it not for the disquieting statements that one of these men is a professor in a college and that such material as this has been used for "a number of years" in the class room.

Indiana University EDMUND S. CONKLIN

Graphs: How to Make and Use Them. By HERBERT ARKIN and RAYMOND R. COLTON. New York, Harper & Bros., 1936. Pp. xvi, 224.

This handbook for the construction and interpretation of graphs should be welcomed by all laboratory teachers, statisticians, and experimenters. It is sufficiently detailed and yet simple enough that even beginning students may be referred to it and should be able to solve their own problems with its help. Best practices have been collected and presented in a series of recommendations which should become standard for all graphs regardless of subject matter. The book is in fact an excellent demonstration of the point that scientific principles are essentially the same for all fields, whether in science or in business.

Users will find numerous excellent devices for pointing out more clearly the real significance of their findings. Chapters include: The principle of the Graph, Con-

struction of the Graph, The Equipment for Graphic Presentation, The Line Graph, The Ratio Chart, The Bar Chart, Area and Solid Diagrams, Graphing Relationships, Map Graphs, Organization Charts, Graphs for Computation, Control Charts, The Reproduction of Graphs, and Statistical Tables and Reports. This volume should be available for all courses in laboratory science, research methods, and statistics.

University of Southern California — ROBERT H. SEASHORE

Prostitution in the Modern World: A Survey and a Challenge. By GLADYS MARY HALL. New York, Emerson Books, Inc., 1936. Pp. 200.

Whether this work presents enough factual data to have psychological merit is difficult to ascertain. Undoubtedly a great deal of study, honest and, usually, unbiased thought have gone into its preparation. Its conclusions, however, are given with a certainty not entirely in keeping with the anecdotal nature of the data. Since the findings were secured through interviews and personal experiences (of the author) in a field in which facts, admittedly, are very much unavailable, they may mean much or little, depending upon the skill of the author and the observational accuracy of the persons (unnamed in the book) from whom opinions were secured. Whether these opinions are descriptive of actual trends, as in the statement that professional prostitution is being supplanted by amateur promiscuity (p. 174), or of changes in awareness on the part of the observers, or both, is, of course, undetermined. The socio-religious attitude that practices not in keeping with legal and religious sanctions are wrong, bad and undesirable, is so much a part of the author's thesis that those who are aware of the sketchy nature of the data relative to sex and sex practices may be inclined to under-value the book. It is, after all, expressive of the opinions of persons intimately associated with the field.

Louisville, Ky. — MILTON B. JENSEN

La Formation des Habitudes. By P. GUILLAUME. Paris, Félix Alcan, 1936. Pp. 206.

The subject of this book is learning in the broader sense, not merely the development of habits. It is a brief review of the problems of learning. There is constant reference to the experimental literature and many of the more important experiments are described in detail.

In the first chapter, the concepts of instinct and habit are discussed. Instincts are not fixed and invariable, but are subject to environmental influences. Habits depend upon instinct and maturation. The experiments of Shepard, Breed, Yerkes, Kuo, Gesell, and others are cited.

A detailed account of the two principal types of habit—conditioned reflex and trial and error—is given. The concept of conditioned reflex is criticized because of the ambiguity of the term and the instability of the reflex. Trial and error implies an unstable determinism, which is contrary to the principles of science. Experiments are cited which show that rats act on 'hypotheses' in solving maze problems rather than by chance. Other problems taken up are the transformation of perceptions in learning, motor education, conditions of habit formation, and interference.

The experimental material is well organized and presented in a clear concise

way. The book presupposes at least an elementary knowledge of psychology. It should prove useful for supplementary reading in advanced classes.

Nyack, N.Y. E. FRANCES WELLS

The Ethics of Sexual Acts. By RENE GUYON. New York, Alfred A. Knopf, 1934. Pp. 410.

This is a philosophico-anthropological volume which is attempting to express the ideas of the World League of Sexual Reform on a scientific basis. The chief theme of the volume is the fact that social custom should be adjusted to the newer knowledge of sex, as indicated by Freudian psychology and the non-mechanistic and psychiatric sexual theories. There is considerable discussion of psychology as a whole, the Freudian viewpoint, as well as the physiology of sexuality. The morality of sexual acts is discussed more extensively from the Freudian standpoint and not quite as much from the anthropological as it is in other similar volumes.

The author also criticizes the psychiatric and the psychological viewpoints toward sex, but not rabidly, and he concludes with a discussion of sexual aberrations pointing out, as so many other valid writers concede that most aberrations are only such when looked at from the Judeo-Christian morality point of view. He advocates individualization of love and while he admits certain weaknesses of this attitude he points out that it is more in keeping with the psychological points of view being held to date than is the attitude of conservative writers. The book is distinctly modern and while not scientific in the sense of laboratory sciences, it is full of fairly well accepted anthropological and psychoanalytic references. It is well written and stimulating. It is not, strictly speaking, a psychological book. However for those who are interested in psychopathology of sex it presents a point of view which should not be overlooked.

Recorders' Court Clinic, LOWELL S. SELLING
Detroit, Mich.

Medical Notes on the Sex Life of the Unmarried Adult. By ROBERT L. DICKINSON. New York, Vanguard Press, 1935. Pp. 28.

Sex and the Love Impulse. By J. H. BURNS. New York, Emerson Books, Inc., 1936. Pp. 61.

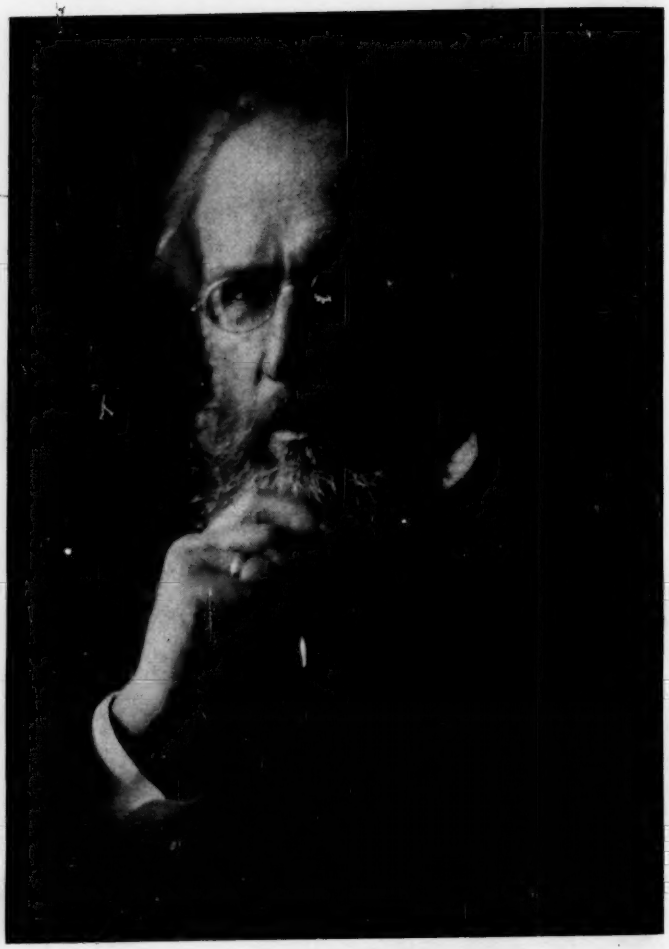
These two pamphlets were probably intended for the information of the lay public, and as such they seem to be fairly sound. The first (a reprint of a chapter in *The Sex Life of the Unmarried Adult,* edited by I. S. Wile) is a discussion of present-day attitudes toward sex and sex practices, as revealed in recent studies, and in contrast to attitudes of a few decades ago. The second pamphlet is a simplified, popular discussion of how a child should be educated to have a sensible attitude toward sex. It includes definitions of a number of terms, and information which the author believes important for sexual adjustment.

University of California NANCY BAYLEY

Human Genetics and Its Social Import. By S. J. HOLMES. New York, McGraw-Hill Book Co., 1936. Pp. viii, 414, 84 figs.

A well written, unbiased, thorough but nontechnical résumé of the facts and theories of inheritance with lucid discussions of their bearing upon problems of mental and social betterment.

Trinity College R. B. W. HUTT



C. Stumpf

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